DOCUMENT RESUME

ED 130 937 SO 009 511

AUTHOR	Chilcott, John H., Ed.
TITLE	Council on Anthropology and Education Quarterly, Vol. VII, No. 3, August 1976. Special Issue: Research Dimensions of Anthropology and Education.
INSTITUTION	Council on Anthropology and Education, Washington, D.C.
PUB DATE	Aug 76
NOTE	53p.; For related documents, see SO 009 509 and 510
AVAILABLE FROM	Council on Anthropology and Education, 1703 New Hampshire Avenue NW, Washington, D.C. 20009 ($1.00)
EDRS PRICE	MF-$0.83 HC-$3.50 Plus Postage.
DESCRIPTORS	*Anthropology; Cultural Factors; *Educational Anthropology; Educational Policy; Educational Research; Elementary Secondary Education; Ethnology; Evaluation Methods; Evaluation Needs; *Financial Support; Government Role; Higher Education; Prediction; Relevance (Education); *Research Methodology; *Research Opportunities; Research Skills; School Role; Social Science Research

ABSTRACT

The collection of papers focuses on the future of research in anthropology and education. Intended as an exploration of the relationship between funding agencies and the individual anthropological researcher, the articles generally question the degree of control which the anthropologist can exert on research. The basic premise of the papers is that anthropology possesses a methodology and a conceptual framework which can contribute to a better understanding of the cultural process of education within a society. The first two papers explore the political considerations of research by investigating federal-agency programs and by measuring the involvement of anthropologists in federally contracted evaluation. The third paper discusses the relationship between educational policy and anthropological fieldwork and concludes that policy makers might be more receptive to research if it is related directly to the premises of a particular policy. The research methodology that has developed in the field is discussed in the last paper. Its author relates that the conceptual framework of educational anthropology possesses a comparative orientation, places a high value on situational variables, and values intensive field study. Professional news, college teaching information, and a bibliography of recent works are included. (Author/DB)

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SEP 2 7 1976

ANTHROPOLOGY and EDUCATION QUARTERLY

Vol. VII, No. 3 August 1976

Special Issue

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RESEARCH DIMENSIONS OF ANTHROPOLOGY AND EDUCATION

INTRODUCTION

John H. Chilcott

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The collection of papers in this issue of the *Quarterly* attempts to focus on the future of research in anthropology and education rather than on past accomplishments. The basic assumption of these papers is that anthropology as a science possesses a methodology and a conceptual framework which when applied to the study of the cultural process of education in a society, can provide a singular contribution to a better understanding of that cultural process, and that to accomplish this goal, individuals need to master the methods and content of the scholarly discipline of anthropology.[1]

Much of the dialogue within these papers is directed at the relationships which exist between the funding agencies and the anthropologist as the individual researcher, particularly with reference to the degree of control which the anthropologist, either as an individual or collectively, can exert within the research arena. The dialogue also suggests that since much of anthropological research within education may ultimately be directed at fabricating educational policy, there exists a political dimension as well as a methodological dimension with which the researcher needs to contend.

The sequencing of the papers has been designed so as to move the reader from political considerations of research toward methodological considerations and prescriptions. This issue of the *Quarterly* is also sequenced to provide an introduction to a forthcoming production on anthropological perspectives of qualitative versus quantitative research in education to be made available to the CAE membership before the end of the year.

Notes

1. For a discussion of the need for researchers to be trained within a discipline, see Harold Howe II, "Education Research–The Promise and the Problem," 5:6 *Educational Researcher* 2, June 1976.

REPORT OF THE ELECTIONS COMMITTEE

SP009511

The Elections Committee convened on 1 June 1975 for the purpose of counting the ballots received for the 1975 election of officers. We certify the following as the results of our tabulation.

Glenn Hendricks, Chairperson
Marion Dobbert
David Zander
Elections Committee

Ballots received	558
Unsigned ballots not usable	9
Total usable ballots	549
For President	
Dell H. Hymes	390
Henry Torres-Trueba	156
Blank	3
For Board of Directors Member	
Henry G. Burger	200
St. Clair Drake	341
Blank	8

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Published four times a year (Feb., May, Aug., Nov.), distributed to members of the Council with membership. Individual membership $7.50 a year (includes $4 for subscription to the *Quarterly*). Library subscriptions $10 a year. Single copies, when available, $1 each.

Send communication on membership to:
Council on Anthropology and Education
1703 New Hampshire Avenue NW
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ANTHROPOLOGY AND EDUCATIONAL RESEARCH: A REPORT ON FEDERAL AGENCY PROGRAMS, POLICIES AND ISSUES[1]

Francis A. J. Ianni
Horace Mann Institute, Columbia University

Over the last decade there has been a steadily increasing interest among educators in anthropology and particularly in the use of field methods in educational research. To a considerable extent, this interest grew out of the Civil Rights Act of 1964 which pushed education, and consequently the research which informs it, into a concern with cultural as well as individual differences. To some extent, it was the result of dissatisfaction among educational research clients who tended to see traditional educational research paradigms as abstractions from the reality of the everyday life of schools. This interest also resulted, however, from the growing sophistication of educational researchers themselves as they began to explore methodologies other than those which developed during education's long and incestuous relationship with psychology. This new interest, however, is not without problems. The demands which are being placed on anthropology by educators are major ones and in many cases they include expectations of problem solution and educational utility which are unrealistic, given the current level of development of anthropological involvement in educational research. At the same time, the recency of educational interest in anthropology and the long tradition of dominance over educational research by educational psychologists has produced a research and development climate which is not oriented to the style and pace of ethnographic studies. Finally, the relative recency of interest in educational research by anthropologists has not allowed sufficient time for systematic development of theory and methods in educational anthropology itself.

Despite these problems and a number of less obvious ones, interest in anthropology continues to grow in education and as a result a number of important policy questions for both educators and anthropologists are now emerging. While it is customary to pose these questions in terms of research programs, they actually fall at every step of the research-decision making-policy formulation-implementation and evaluation continuum, which is the basis for educational innovation and change. Thus, the uses to which anthropological data will be put, the means by which it will be assessed and evaluated, the effects it may have on education and on social policy, and the results of all of this on the discipline of anthropology itself are of equal importance.

Early in the spring of 1974, we interviewed a number of program specialists and managers in various federal government agencies which have a concern with education in an attempt to uncover what place anthropology holds in their research programs, what issues and problems seem to have emerged or are impending in this experience, and what they see as potential applications of anthropology in their mandated areas of concern. In presenting some of the results of these interviews in this report, we have first chosen to describe the current status of anthropology in a number of educational or education-related research programs in the federal government, and then to describe what the policy issues seem to be. We also elected to deal with the area of evaluation as separate from research both because we feel it presents different issues and because it is placing more urgent and widespread demands on the discipline.

While we have characterized the interest of educators in anthropology as of fairly recent origin, there was some interest in anthropology for a number of years. The National Defense Education Act of 1958, for example, brought considerable interest in area studies as a means of preparing students as area specialists. It is interesting, however, that this had to be justified to the Congress as part of the "Cold War" preparations for military defense. Historically, aid to education, and consequently funding for research intended to improve education, has always had to be "categorized." The traditional Congressional distaste for general aid to education has been reflected in the need to present research programs in very specific, problem-oriented packages. Thus, when the Cooperative Research Act of 1964, the "discretionary" research program out of which all subsequent Office of Education (OE) and National Institute of Education (NIE) research programs have grown, was first established, three-fourths of its available monies were ear-marked by the Congress for research on the education of the mentally retarded. In the mid-1960s, however, the OE decided to try some new "focused" research programs, largely as a result of the fact that the then-new National Science Foundation (NSF) was making considerable progress in the area of curriculum development (and attracting considerable Congressional notice as a result) while the OE was doing little more than dispensing money along formula grant, non-discretionary lines. Again because of it. concern for Congressional suspicion of general aid, however, the new programs had to be constructed around categorical areas of educational concern. One of these was called "Project

Social Studies" and had the avowed purpose of improving the teaching of social studies in the public schools. Under this rubric, a small research program on the culture of schools was funded by OEs Cooperative Research Program in 1964 as a means of attracting anthropologists to the study of education. It was from this modest beginning that current programs in both the OE and the NIE, which has taken over most of the OEs research functions, have grown. Other agencies, such as the National Institute of Health (NIH) and the NSF, have come to an interest in educational anthropology by a different route resulting from their interest in anthropology as part of their mandated areas of concern rather than having a direct concern with education. Here again the interest in anthropology and education is both recent and indistinct in focus.

Even today, anthropological involvement in federal educational research program planning is more a matter of potential than reality. Dozens of divisions and offices in at least six different agencies have expressed interest in the possibility of anthropological proposals concerning educational problems. Some of these offices have already funded anthropological research but not on education; others have concerned themselves with educational issues but not from an anthropological perspective or methodology. Only a few (compared to the more extensive involvement in evaluation areas) have actually had proposals from educational anthropologists up to this time; a number of these have been funded. Anthropologists seem to have been minimally successful in formulating their proposals and rather late in exploring the governmental funding structure so that, in 1974, the NIE (the most obvious source of funds) was glutted with a multitude of proposals as a result of announcing a special program to encourage anthropologists to apply but found that it could fund only a small fraction as a result of drastic budget cuts.

For this report, we conducted interviews in six agencies–the National Institute of Education (NIE), the United States Office of Education (OE), the National Science Foundation (NSF), the Public Health Service (PHS), and the National Endowments for the Arts and the Humanities (NEAH) as sources of educational anthropology funds.[2] In brief on research programs, we found that a number of opportunities for developing liaison between the federal research agencies and educational anthropologists still exist. Indeed, there are considerably more opportunities than there are examples of their utilization. Most research applications in anthropology and education now seem to be directed to the NIE despite the low rate of return. To some extent, this is inevitable since most anthropologists have broad, basic research interests and the current watch word in Washington is "applied." Anthropologists in general should explore a broader variety of federal agencies and should exercise more effort to be both specific and to explain their discipline. An understanding of research methodologies commonly employed by anthropologists and their rationale, the types of results they yield, and the like, simply cannot be assumed. In addition, anthropologists must face the issue of the limits imposed on their research by their tendency to work alone rather than in teams. Quite aside from the often idiosyncratic data produced, this means that only relatively small populations can be dealt with. To deal in considerable depth with even a single school, however, will require dividing the task into several components and the cooperation of a research team in their completion. Despite these problem areas, many federal program administrators are quite interested in the possibilities of relating anthropological research to their various areas. After a bit of explanation of the sort of problems anthropological research can broach and the data it can develop, many were quite enthusiastic, indeed.

Evaluation

We also spent considerable time interviewing program specialists and managers at the OE and the NIE on the growing interest in anthropology among educational evaluators. Evaluation has become a major activity of all social remedy programs; within education, it plays a leading role in research and development activities. Governmental agencies, the Congress, and even the public are concerned with educational activity and its effectiveness. As demands for good evaluation increase, however, so does the impact of poor evaluation. New demands and concerns, both of the programs being evaluated and the agencies to whom evaluators are responsible, have brought about the current widespread dissatisfaction with past methodology. The customary use of quantitative data, for example, may provide much useful information on student achievement but there is growing evidence that it can't supply satisfactory answers to many of the qualitative questions of education today. It is no longer enough to say that Johnny can't read; what is now being asked is why he can't and what will make him learn.

Because of increasing dissatisfaction over current evaluation methods, two new attempts to refocus the more traditional types of evaluation have emerged, both with important implications for anthropology. One has been a swing from a fixed focus on the individual as learner to an indistinct focus on the program as the unit of evaluation. Early evaluation designs placed the major emphasis on psychometric testing of students. The design was as simplistic as it was rational: if the goal of the program was to familiarize students with a specific body of information, then the best way to determine the success or failure of the program was to test those students to see if they had indeed acquired that information. So long as evaluation played a minor role in education, largely restricted to research concerns, the method did not present problems. In the 1960s, how-

ever, educational consumerism became a prominent movement in education and older evaluation methods no longer sufficed. Billions of dollars poured into education programs for the culturally deprived. Ghetto parents alleged that standard tests were culturally biased. Effectiveness of the tests in measuring program success was challenged. A major confrontation came when the Westinghouse Learning Corporation gave poor marks to the popular Head Start program because achievement tests indicated little significant student progress. The Head Start program, then popular both in Congress and in urban communities, remains popular today. The challenge to the program presented by the Westinghouse Corporation, met by sharp public criticism of the company's evaluation standards, threw current evaluation techniques into the arena of public debate. For the first time, evaluation methodology was seriously questioned by people outside the profession. Evaluators were forced to shift away from focus on the individual-as-learner and "program" evaluation became a major area of concern. In order to evaluate programs, observation was essential and agencies began looking to anthropology and "soft" sociology for techniques of field observation.

A second attempt to refocus evaluation design also came about when social remediation programs began to acquire great quantities of money. Social service agencies joined the ranks of the well-to-do. When the passing of Title I of the Elementary and Secondary Education Act of 1965 brought the sudden investment of one billion dollars into education in the inner city, Congress became concerned over how the money would be spent. It was the attachment of an evaluation requirement to the Elementary and Secondary Education Act which led both to the rapid growth of evaluation and evaluation agencies and to one of its current characteristics as a system of financial monitoring attached to any grant, award, or contract which proposes to produce change in education. Because so much of the new money in education and in educational research was pumped into urban (and primarily urban ghetto) areas, questions of the effects of culture, sub-culture, race, ethnicity, and a variety of other features of the community now became extremely important. The established procedures of close experimental control, control groups, and other features of the logic-deductive process began to falter as these new concerns emerged. Clearly, the laboratory-oriented style of research which was part of the tradition of educational research was not satisfactory for the evaluation of either ethnic studies programs or affective programs of education. Again anthropology, which had always been concerned with these issues, provided a potential source of methodology.

Not all of the pressures for change, however, came from outside. Within education, there was growing dissatisfaction with evaluation studies that resulted in a summative black book at the end of a program which said to program developers, "Here is what you have done; here is what was wrong with it; if you ever do it again, here are some suggestions which you might want to follow." In new evaluation procedures, evaluators were required to abandon the approach of scientific detachment they had adopted and "formative" evaluation became a part of the evaluation lexicon along with "summative" evaluation. Rather than being above and beyond the program, the evaluator was compelled to become a part of the program. Data were acquired at the side of a program or in cooperation with it.

Most educators and many educational researchers agree that modern evaluation technology is in a sorry state. We still tend to place it under the rubric of research—thereby forcing upon it canons of science which are in many cases not applicable. Most evaluations today are not research but rather *ad hoc* attempts to provide some basis for describing and assessing programs and accounting for expenditures of funds. Methods are almost indiscriminately pulled from a hat or, rather, from a number of hats. Methods of testing come from psychology, techniques for program monitoring are borrowed from systems analysis techniques, methods of qualitative analysis are adopted from sociology and anthropology, and all of this is hurriedly thrown together to make a bidders deadline. Despite the fact that we persist in referring to evaluation as research, none of the requirements of sound research seem to operate here. There is no theory to inform methodology, there is no consistency in methodological development, there is no systematic application of methods, and, perhaps most important, there is no system by which what is learned in one evaluation informs the next evaluator.

The primary reason for the discrepancy between evaluation means and ends stems from education's failures to develop a consistent evaluation methodology. Traditional techniques have been formed within a framework culled directly from the various behavioral sciences. The reality of education, however, may diverge sharply from the abstracted systems of the behavioral disciplines. And methods are normally adopted without reference to their supporting concepts or theory.

The dangers of methodological transplant are not immediately obvious to most educators. Faced with an evaluation requirement, educators turn to behavioral scientists for a scheme which will allow them to assess their program. Yet, because behavioral scientists deal with specified conditions, their methods are usually applicable only where certain conditions can be reproduced. Such closed system models also must deal with a discrete number of selected variables. While such models are appropriate to the study of some educational issues, most educational programs which are designed to produce change require a more open, qualitative, analytic

framework. Thus, while most current methods of gathering, recording and analyzing educational data are based on experimental models and require statistical-quantitative measurements, most educational encounters are non-experimental (that is, they are goal-specific operational field engagements), and existing methodology yields little insight into the nature of these encounters. The problem is further complicated because while they are told to control certain variables to see what happens to others, the educators' task is to manipulate variables to achieve multiple objectives; some of these objectives are more important than others, and some may conflict with others. As of now, they have no way of arraying these objectives and evaluating differential effects by manipulating combinations of variables. Nor do they have a systematic procedure to assess the costs of accomplishing various effects. There is a growing tendency to turn to anthropology for guidance in developing sound program evaluation.

It would be tempting to suggest that anthropology does provide a coherent set of methods and a theoretical base out of which educational evaluation might develop a consistent conceptual framework and methodology. Such is not the case. It would be even more comfortable to suggest that evaluators could look to psychology for testing, systems analysis for monitoring, anthropology for field research—but this would produce more problems than it would solve. In the first place, the task of examining, defining, and redefining the institutional setting in education should be the principle responsibility of the craft itself, and so it should not be assigned to some outside agency or group, no matter how willing they are to assume it and how unwilling educators are to attempt it. Second, there are no existing preordained methods growing out of specific theories through which evaluators can approach the task of evaluation with assurance of success. There is no theoretical pattern for observation and analysis in any of the existing research strategies of the behavioral or social sciences which fits evaluation needs. Nothing emerges so clearly from "inter-disciplinary" research ventures as the obvious fact that each discipline of the behavioral and social sciences has built its own conceptual framework and culture. Each is characterized by preferred ways of looking at and into the world. These preferences are not superficial; they characterize the kinds of questions asked, the ways in which answers are interpreted and presented. Each discipline has its own *patois*, a mixture of generalized meanings from the social sciences, and its own peculiar and sometimes precious jargon.

Again, there are fundamental differences between the analytic frameworks of the behavioral scientist and the educator. Typically, behavioral scientists are interested in a discrete number of aspects of the empirical phenomenon they are studying, and so they deal abstractly and selectively with a small set of variables. Educators, on the other hand, must work in the real world, where they must recognize a number of variables in the situation. Realistically, there is no analytic or conceptual framework which is peculiarly developed for education, and so no framework for evaluation. Thus, when we should be asking for perspectives from the behavioral sciences within which to frame questions growing out of behavioral encounters in education, we ask for answers and the comfort of borrowing and adapting a conceptual and analytic framework within which to fit these answers.

All of this—the movement from individual to a program-based focus for evaluation; the concern with qualitative rather than exclusively quantitative assessment techniques; the impinging of culture, ethnicity, and community on educational programming; and the need to observe all of this in a field setting—has been pushing evaluators away from educational research's traditionally incestuous relationship with psychology towards some active courting of anthropology as a possible source for evaluative technology. In one respect, educators' problems are comparable to those confronting anthropologists. They, too, must observe, record, and analyze behavior in the field, not in a laboratory setting. Through the development of a conceptual model and correspondent methodology, anthropologists have achieved considerable precision in a natural environment. Over the last several years, this similarity of field strategy has led to increasing interest in what has come to be called the "anthropological method" or, more frequently, "anthropological approaches" to evaluation.

While the phrase "anthropological approach" conjures up a nice image of evaluators as resident ethnographers describing the culture of the systems they study, it is not only inaccurate but is dysfunctional because it perpetuates a number of growing misuses. In the first place, "the anthropological approach" has come to mean specifically the use of participant observers, in a very simplistic equation which says nothing about how participant observers are used. Participant observation is an important research style in anthropology (as it is in a number of other social sciences) but within that style it is necessary to develop skills in the use of techniques. Most of the evaluations we have seen which propose to use participant observation usually stress the observation to the exclusion of the participant role, and in many cases there is no clear definition of what is being observed and, more important, how it is to be observed, how it is to be recorded, and to what end this is all being done. The use of untrained participant observers, who have no grounding in theory and who have not learned the difference between looking at and looking for, has created problems in the field as well. School teachers and community residents have also created their own "conspiracy" theory of evaluation. More and more, teachers and community people are viewing evaluation

as a device for maintaining the system as it now exists. Evaluation is done, in this view, to inform educational decision-makers so that they can suppress any moves for change. When teachers are evaluated, like anyone else who was ever evaluated, they naturally become uncomfortable. It is easy to assuage this discomfort by criticizing evaluators. When participant observers who don't really seem to know what they are doing appear on the scene, the teachers' attitude is exacerbated and their criticism is fueled.

Finally, most of what we have said earlier in this report about the confusion between behavioral science research as a value-free style of data gathering and analysis is even more true in evaluation which is, of necessity, value judging. While anthropologists are interested in the scientific study of values, they set great store by the fact that they note their analytic skills as free of judgmental bias. In addition, while anthropological techniques appear at first blush to be almost simplistic (as one curriculum specialist told us "I have been using the anthropological technique for years—I always visit my schools"), they are closely tied to conceptual frameworks which inform the methodology and are much more difficult to master than survey research or questionnaire approach *because* they are much less structured. All of this is to underscore our major point that field research techniques hold great potential for use in evaluation but that this should not be confused with anthropology. Field work techniques are used broadly in anthropology as well as in sociology, and in both cases they are related to theory and cannot be used apart from that theory.

One of the major reasons why field research techniques are so attractive to educational evaluators is because they allow for the gathering of vast amounts of descriptive data about the schools, the personnel and students and the community. Unfortunately, this very richness sometimes destroys the utility of the data because there is so much, quantitatively and qualitatively, that it cannot be used. What we have said earlier about the need for a conceptual framework within which to develop a strategy for field evaluation is particularly pertinent at this level.

There is also a reluctance on the part of many anthropologists to become involved in educational evaluation, particularly when, as Solon Kimball once pointed out, educational research itself is often used as a guise for attempts at educational reform. Anthropologists, especially since the Camelot affair, are skittish about involvement in governmental reform programs and the spectre of Dr. Strangelove hangs heavy in the field. In summary, then, we are suggesting that the rubric "anthropological approach," while an attractive one, is neither accurate nor productive in describing the application of fieldwork techniques to educational evaluation, and that it becomes an important task to borrow from sociology as well as anthropology those pieces of technique, along with their supportive conceptual frames, which are best suited to extract the rich descriptive data which they can supply for use by decision makers. The reasons are summed up in the following passage:

> *Anthropologists experience the processes, structural variety, and problematic aspects of teaching and learning by intensive, first-hand observation of what goes on in schools and in less formal situations of instruction. Yet few anthropologists otherwise identify with schools. There are school psychologists, a growing number of school sociologists, but no school anthropologists. The demand, and possibly the fashion, for anthropological knowledge and insight in American schools shows no sign of slacking; yet most anthropologists are content to let the popular conception of what anthropology is, how it is done, and what it has to say stand. Education, formal and informal, is the chief medium of the transmitted behaviors anthropologists call "culture," the fundamental idea of the discipline. Yet relatively little is anthropologically known of American public schooling, such related phenomena as "street academies," and the extent to which cultural assumptions and culturally defined expectations bias the American teaching and learning experience.*
>
> *These apparent contradictions derive from, first, the traditional insistence of anthropologists on research in societies other than our own; second, the numerical scarcity of anthropologists; and, third, from conventional attitudes of the profession that consider the practical effects of anthropology to be secondary to its theoretical advance. Serious and sustained application of the discipline to the human opportunities and dilemmas of teaching and learning is anathema to most anthropologists. Much impetus for including anthropology and anthropological perspectives in our schools comes not from the profession of anthropology but from interested public (and private) school personnel.*[3]

All of this sustains a fairly delicate balance between anthropology and evaluation, as it does with educational research at this point. Again, there is growing interest in "field research," "ethnographic approaches," "the anthropological approach," and a number of other intrusions of anthropology into educational research and evaluation; the growth seems destined to continue unless some group or groups within the profession decides to attend to the growth and nurture of the field.

Policy Questions and Issues

The issues which have already been presented earlier in this report, and others which developed as we

discussed the interviews, present some important policy questions which should be discussed and, we hope, resolved at some point if the considerable promise which educators see in closer contacts with anthropologists are to be realized. In essence, these policy questions are: (1) the appropriateness of anthropology for the study of education, (2) the relationship between anthropology and social policy in education, and (3) who speaks for anthropology?

The Appropriateness of Anthropology for the Study of Education

As we pointed out early on, there is a growing tendency in education to turn to anthropology for research methodologies in both research and evaluation. While this has been most characteristic of the National Institute of Education, the several other agencies we interviewed are also beginning to turn in that direction to the extent that they are developing an interest in education. In each case, one senses there is almost the expectation that anthropology will somehow provide a research paradigm which will erase the long tradition of ignoring the social context of education. In large measure, this results from the fact that studies in education have tended to focus on the individual as learner to the exclusion of social context until the Civil Rights Movement forced such attention. But even then education failed to look at the social organization of education as a natural setting for learning, and most organizational theory and methodology in education still comes from a concern with administration and management and so intersects with the sociological literature on formal organizational analysis. As a result, schoolpeople tend to question the fit between the formal analytic models displayed by educational researchers and the social reality of their own schools. And what is operationally more important, they can't see how they can get from their "here" to the organizational analyst's "there;" many are not certain they would want to make that trip. This is, we think, one of the major reasons for the growing movement to apply the techniques and conceptual methods of anthropology to educational research. We hope this new look may (and probably can) provide better data on how schools actually function, as well as serving as a tool for developing models for change in educational systems that earlier methods could not provide.

Nonetheless, despite this new interest, conventional research and evaluation procedures in education continue to exploit arbitrary environments, statistical measures, and "problems" in seeking information (often basic) about what goes on in educational encounters and in building knowledge of educational processes and structures. Anthropology does offer a considerable expertise to traditional educational research in this regard. One distinction often made for anthropological inquiry is that it describes "natural" environments and ground-level behavior. Thus, the descriptive activity of anthropology should concern what people are observed to do, not simply what people say they are doing or what they claim ought to be done. Certainly it would be difficult to argue that anthropology is not a discipline of considerable worth to the study of education. But, given the current problem-solving emphasis in educational research, there is some question as to whether anthropology's preferred style of the solitary field worker who performs every step in the research process as an individual and its resistance to large-scale team research are valuable and, if so, how much. Though the craft of anthropological inquiry may apply to educational research, it is also true, however, that a great deal of the anthropology of education still answers mostly to the profession of anthropology. Many anthropologists whose primary interest is the anthropology of education still have a marked tendency to assess their work as anthropology; they have, in their view, a prior and fixed commitment to the discipline and to whatever will advance disciplinary interests, theoretical or practical. This is, of course, an important and worthwhile undertaking, particularly since the history of educational innovation and, indeed, all recent attempts at social remediation indicate so clearly that when social action programs do not grow out of and re-inform some body of theory, they seldom produce any institutional change. If anthropology is to realize its value to education, it must also apply its attention to questions of both theory and utility in education. Most research administrators really don't see any immediate utility of anthropology to problem-solving (as contrasted to problem-defining and problem-describing) strategies in education.

To some extent, this attitude results from the nature of anthropological inquiry which is holistic, situational, descriptive, non-analytic, and generally designed to result in a statement of system characteristics rather than of the inevitable association of the elements within the system. Eventually we produce a typology of systems and any generalizations which can inform practice are dependent upon the ability to identify the operational system with one of the model system types. Schoolpeople, socialized in schools of education to a tradition of educational research which was highly analytic and which purported to present interrelationships among elements of a system which were conceptually independent of any given situation, want statements of law-like regularities from research. Thus, when ethnographic accounts of schools are presented to urban school principals, they cannot (and probably should not) proceed to make operational changes in their schools based upon these data. Most educational administrators still regard anthropological studies of schools as insightful empathetic descriptions which they do not trust because they are so understandable; inevitably, they

send the studies on to the social studies teachers for classroom use. So, the problem is not just a question of introducing anthropological methods or perspectives in educational research, but of convincing both research administrators and other educational researchers that anthropological research and its interpretation and validation operate within a different but equally reliable and valid climate from other disciplines.

All of this suggests that the current usage of anthropology by educators, and particularly in educational research, does not meet the real test of adequacy: is there a clearly denoted area of interest and competence within educational research which is subject to control by anthropologists? Our answer is no for a number of reasons which we delimit later in this report. For now, it suffices to say that anthropologists are still involved in education as individuals, and usually as consultants on major research and evaluation projects. Government agencies explain this by citing the individualistic style of anthropologists ("our experience is that anthropologists not only prefer to work alone, they are actually disruptive if you mix them with other social scientists or educators") but that team research is impossible in anthropology ("we seldom ask more than one anthropologist to work on a project because they seldom agree with each other").

Certainly some of the reservations of research administrators about the uses of anthropology (and of anthropologists) in educational research are realistic. Our own experience in team research, for example, convinces us that if the old axiom—the problems encountered in team research increase numerically with the number of scientists involved—is true, the problems seem to increase geometrically when those scientists are anthropologists and exponentially when they are anthropologists mixed with other social scientists. Be that as it may, the important question is not so much one of the adequacy of anthropological inquiry for educational research but how well research administrators and other educational researchers comprehend both the perspective and the methods of anthropology. As we conducted interviews, we became convinced that what is essential is to provide some consistent, coherent information to agencies on what anthropology is and isn't, and what the potential of the field is and isn't for educational research. Not only is there a tendency to overlook some of the potential uses of anthropology, there is an even greater tendency to expect too much too quickly in the form of instant ethnographies, two-week training courses for participant observers, and any number of other requests which indicate a lack of familiarity with the style and method of anthropology. Because there is not a consistent and coherent approach on our part, each agency deals separately and individually with anthropologists and with others who propose "an anthropological approach."

From our interviews, we see four major issues related to the question of anthropology's adequacy as a source for method and theory in educational research:

(1) Are the needs of educational research, which today generally want a product rather than knowledge, well served with a high-risk, low-yield venture resulting in more hypotheses raised than resolved, a characteristic of most good ethnographic studies?

(2) Conversely, would the process of adaptation to governmental needs be disruptive to the healthy growth and nurture of what is still a developing field?

(3) Would it be possible (and profitable) to develop within educational research circles and among government program managers an appreciation of the essential unity of anthropological style—as distinct from other disciplines—so that there might be a genuine adoption and adaptation rather than the present piecemeal borrowing?

(4) Is it possible to mesh the individualistic, highly personalized style of anthropologists with the growing tendency to develop large-scale, multidisciplinary team research and evaluation ventures in education?

What is the Relationship between Anthropology and Social Policy Questions in Education?

From our interviews and from prior experience, it became obvious that policy research is not only a major interest among government agencies but is the most productive and timely means of influencing educational decision-making. Most research in this area has come from economics and, more recently, systems analysis techniques, and is inevitably founded on the input-output model, the basis of most policy research.

Yet much of the debate and many of the policy questions which currently produce tensions in the educational system are actually the result of unrecognized or, at least, unresolved conflicts in values. Questions about whether schools should be integrated or segregated by race, by religion, by social class, or by intelligence are obvious value questions. But many other issues such as the financing and organization of schools, their management and staffing, and the delivery of educational programs are less obviously but probably even more fundamentally grounded in value questions. Present approaches to policy study in education have usually tended to ignore the question of values and their role in structuring policy questions. In part, this results from the intellectual origins of most policy scientists, who have come from economics, political science, systems analysis, or operations research backgrounds. While the concept of "values" is present in some form in each of these areas, it does not have the centrality which it does in anthropology. More importantly, there is no

tradition of research methodology designed to deal with and inform the concept in these areas, as there is in anthropology. In each of these areas (but not in anthropology), there is a real or assumed lack of precision in dealing with qualitative issues such as values, so current policy analysis has tended to develop out of a quantitative methodology. Neither are the clients of policy studies in education particularly attuned to the kinds of feedback they are likely to obtain from qualitative anthropological inquiry. As a result, there has been virtually no involvement of anthropologists in policy research, and there is even an implicit assumption in most agencies that "since anthropology is descriptive rather than analytic," there is no potential role for the anthropologist. It will be necessary for anthropologists to convince government agencies and the public of our relevance and usefulness so that other federal project administrators will learn not to make the statement one did during our interviews: "Well, I don't think we would have much use for anthropologists. You see, our office is concerned with matters of public policy." Two important issues emerged in our interviews:

(1) Are there important contributions which anthropology can make to policy research (a rhetorical question, obviously), and what is the best mechanism for furthering anthropological involvement in this important area?

(2) Other than involvement in research, are there other means of insuring that anthropological concerns and interests are represented in policy- and decision-making in education?

Who Speaks for Anthropology?

Finally, there is the question of who speaks for educational anthropology. Phrased differently, this question asks to whom government agencies will turn in making the kinds of policy decisions which must inevitably affect research related to anthropology and education. To give some notion of how important this question is, some experiences during our interviews are indicative of the problems presented by the distinct nature of anthropological inquiry. During the course of interviewing staff members of the Experimental Schools Program at the National Institute of Education, we were asked for some help in thinking through a problem which had arisen there and which is certain to arise elsewhere. The Experimental Schools Program has been actively courting anthropologists to work on its evaluation programs and, in fact, probably has more anthropologists under contract than any other agency involved in educational change programs. As part of the evaluation studies, a number of fieldworkers are gathering in-depth ethnographic materials on schools, their communities, pupils, teachers, and, in some cases, on the evaluators themselves. These data are typical of what fieldworkers usually gather: highly personal accounts which include information given in the field in the confidential relationship which fieldworkers establish with the people they are studying. Some of the materials are not only highly personal, they are potentially damaging or at least embarrassing to the people under study. It is impossible, of course, to give anonymity to the principal of a school, to a teacher who is so fully described in field notes that pseudonyms are preposterous, or to student activities which contravene school rules or even violate the law. The specific problem for the Experimental Schools Program involves the confidentiality of field notes. Should all field notes from Experimental Schools Evaluations become part of a general data bank, available to all researchers in the same manner as results from more traditional educational research? Can anthropologists (citing Section 51:370 [1949] or subsequent sections of the March 1967 Statement of Ethics of the American Anthropological Association) refuse to turn over raw field notes (are these the same as the educational psychologist's "data") to the government agency supporting their research? The questions stretch out beyond these, particularly when one remembers how paranoid many of us are about letting anyone see our field notes. The point of this anecdote is that, while similar questions dealing with research by educational psychologists would automatically be referred by the agency to the American Educational Research Association, there is some question as to who speaks for the sub-discipline of anthropology and education. There are at least five organizations that we know of which might lay some claim to representation here: The American Anthropological Association, its component Council on Anthropology and Education, the Division of the American Educational Research Association, the National Academy of Education's Committee on Anthropology and Education, and, conceivably, the Society for Applied Anthropology as well. This is not a question of jurisdiction, but one of the lack of a clear voice in speaking to and with government agencies on matters affecting anthropological research related to education.

While such questions as research ethics and standards are important, there are other issues here as well. There is still some feeling in government agencies that anthropologists who have made major commitments to education are somehow less than first-rate. This attitude, characteristic of the earlier attitudes toward educational psychology and educational sociology, stems partly from the common thrust of government agencies to get beyond the current level of research in an area by luring in "the best minds" in the field. Realistically, however, it is also present because there is as yet no established field of anthropology in education as there now is in educational psychology and educational sociology. One of the usual arguments for the development of such sub-disciplines is the inadequacy of existing disciplines

to deal with the complex of problems, the nuances of interpretations, and the utility of implementation schemes in some domain of human behavior. Certainly this can be demonstrated to be true of the area of anthropology and education. But we would argue that the development of a coherent sub-discipline of educational anthropology still requires: (1) the prior development of a conceptual framework out of which problems can be identified and questions posed; (2) once these problems and questions have been phrased, there must be some consistent methodology or methodologies which are compatible with these questions and which are capable of producing the kinds of data which can inform knowledge generation and decision-making in that area of specialization; (3) that once the knowledge and subsequent policy formulations are placed into operational usage, some scheme of appraisal must accompany them which promises to re-inform research and theory in that area; and, finally, (4) since we subscribe to Redfeld's notion that the real hallmark of an academic discipline is that it has its own mythology and kinship structure, that some provision for the systematic training of new personnel be an integral part of the overall scheme.

Our own experience, reinforced by gathering data for this report, is that none of these conditions prevail in anthropology and education today. As a result, encounters between anthropology and educational research tend to be episodic and highly situational. Despite the fact that there has been a consistent development over the last few years of ethnographic materials on schools, there is little accumulation of knowledge since each new study profits little from previous studies. The issue then is one of developing the field both conceptually and in terms of new students. There are still, as far as we know, only two institutions (Teachers College and Stanford) which offer formal programs in anthropology and education.

Two major issues require statement here:

(1) Is it possible to identify or establish some locus for continuing concern with the field of anthropology and education which can deal authoritatively with the important policy and research questions beginning to arise and in which anthropology's interests may be quite different from other disciplines?

(2) Are there means by which new program thrusts—fellowship and traineeship funds, career scientist funds, and other means of non-project funding—can be established in government agencies in order to give to the new field of anthropology and education the same opportunities for systematic development enjoyed by more established disciplines in the past?

There are, of course, any number of other issues which might be cited but these appear to us to be the most important ones which emerged during the interviews. Like all issues, they lead to a number of possible courses of action for remediation. From our interviews and subsequent discussions, we concluded that the sooner such issues are discussed and recommendations developed by the CAE for some action, the sooner some consistent development within government agencies vis-a-vis the anthropological study of education will take place. Left to their own devices, government administrators, with the best of intentions, will continue to deal piecemeal with the field.

Notes

1. This is an abridged version of a report prepared for the Committee on Anthropology and Education of The National Academy of Education. Craig Jackson Calhoun and Elizabeth Reuss-Ianni assisted in gathering information from federal agencies.

2. In the original version of this report, specific program concerns, funding patterns, and research interests in each agency were detailed here but have been excluded from this version due to limitations of space.

3. Francis A. J. Ianni and Edward Storey, *Cultural Relevance and Educational Issues: Readings in Anthropology and Education*, Boston: Little, Brown, 1973.

APPLIED ANTHROPOLOGY INTERNSHIP AT UNIVERSITY OF SOUTH FLORIDA

With a grant from the National Institute of Mental Health, the University of South Florida, Tampa, recently instituted an internship project as a part of its master's program in anthropology. The purpose of the project is to develop internships as a method of training applied anthropologists to work in mental health and related fields. The NIMH grant provides stipends for students during the periods of internship (usually with a local human service agency or institution) and thesis preparation in the urban and medical areas of the Anthropology Department's MA program focusing on the applications of anthropology. The project director is Alvin W. Wolfe, Coordinator of Internships for the department. Other faculty members include Michael V. Angrosino, Ailon Shiloh, and Curtis W. Wienker (medical), and Gilbert Kushner, J. Jerome Smith, Patricia Waterman, and Robert M. Wulff (urban). Gilbert Kushner is department chairperson, and Ailon Shiloh is director of graduate studies.

THE INVOLVEMENT OF ANTHROPOLOGISTS IN CONTRACT EVALUATIONS: THE FEDERAL PERSPECTIVE[1]

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Participation in federally contracted evaluation research is, by definition, a unique experience for most academically based social scientists.[2] Bernstein and Freeman (1975), in an extensive review of federally sponsored evaluations for fiscal year 1970, noted that only 31% of the awards for evaluation studies went to persons in universities or university-affiliated centers. Furthermore, these authors reported that of the awards to university-affiliated scholars, only 44% were contracts, as opposed to grants. The funding patterns described by Bernstein and Freeman noted that as budget size increased, the percentage of awards that were contracts increased, and the number of university-based scholars involved decreased (see Table 1).

Table 1
BUDGET SIZE AS A FUNCTION OF TYPE OF AWARD

Budget	Contract	Grant
$10,000-49,000 (n59)	27.1% (16)	72.9% (43)
$50,000-99,000 (n57)	35.1% (20)	64.9% (37)
$100,000-149,000 (n47)	44.7% (21)	55.3% (26)
$150,000+ (n60)[1]	68.3% (41)	31.7% (19)

[1] Thirteen cases omitted because of lack of information.
Note: $X^2 = 23.01$, $p < .001$

Bernstein and Freeman characterized two types of individuals involved in federally sponsored evaluation studies. They were: (1) Academics—characterized by university-based academicians awarded grants, projects with small budgets and extended times (lasting two years or more), staff members who define the major audience of their efforts as academic, and projects guided by some formal theoretical framework; and (2) Entrepreneurs—characterized by profit-making corporations awarded contracts, projects with large budgets but relatively shorter time spans, staff members who perceive themselves as serving the government and thus defining the sponsoring agency as the *major* audience for their efforts, projects usually employing persons with less advanced degrees, and projects that are less likely to evaluate programs guided by some theoretical framework.

Given this characterization of the dichotomy existing in federal evaluation studies, it is not surprising that those few academic types who have wandered, by chance or by choice, into the world of large-scale federal contract research and evaluation have expressed concern about clarifying their roles (Everhart, 1975); some have even expressed a measure of "cultural shock" (Nelson, Giammotta and Lundin, 1974).

Indeed, this dichotomy between academics and entrepreneurs may be useful in placing a perspective on some of the difficulties currently facing anthropologists participating in federal educational evaluations. Despite the boom in evaluation technology in the 1960s, the tangible results of evaluation studies had fallen far short of the expectations of practitioners, researchers, and policy-makers. At the heart of much of the criticism was a rejection of the simplistic input-output evaluation models in vogue at that point. Reformists called for a greater illumination of variables that describe the "process" of education instead of only the "outcome" of education. The methods employed by anthropologists seemed to hold out the promise of that illumination. Federal agencies proposed that an increased understanding of the context of instruction would assist in interpreting the impact, or lack of impact, of the innovative educational programs they were sponsoring.

Unfortunately, when federal agencies, in the early 1970s sought anthropologists experienced in educational research, and at least familiar with the world of federally contracted evaluation, they found an almost complete void. The pool of prospective anthropologists was almost exclusively based in universities. Those few who could be attracted to join such "new breed" entrepreneurial efforts soon found that they were ill-equipped and ill-prepared by their academic experience for the realities of federally contracted evaluation.[3] After a relatively short period of participation, this frustration inevitably led both partners in the relationship (anthropologists and federal officials) to question the degree of congruence between the traditional federal patterns of conducting evaluative research and the principles, premises, and traditions of the discipline of anthropology.

Everhart (1975) has urged modification of traditional

federal strategies of contract evaluations so as to accommodate the "fieldwork" orientation of anthropologists and sociologists. That is, indeed, good advice. As ill-prepared as anthropologists were for federal contract research, the federal agencies, particularly educational research and development ones, were equally ill-prepared to interact with and understand the anthropological perspective. Though the Everhart proposal has merit, there are nevertheless real limitations to the amount of modification federal agencies can tolerate and still remain alive in the volatile political matrix of Washington.

The Responsibilities of Federal Agencies

The uniqueness imposed on evaluation studies sponsored by federal monies is, for the most part, a function of the responsibilities assigned to the federal agencies. There are three main elements directly related to the evaluation efforts: (1) to foster socially relevant research and development programs; (2) to provide timely input for policy-makers; and (3) to maximize the returns of efforts conducted with limited fiscal resources.

These aims represent the formal "charter" of government agencies. Collectively, they form the framework to which agencies are held accountable. To remain politically workable, federal agencies have very little flexibility to make compromises which would be in direct conflict with them. In the real world of federal research and development, these responsibilities are constantly tempered by political fluctuations as well as by interagency rivalry. Nevertheless, they do represent the basic premises against which all research and development efforts must be judged.

Probably the clearest Congressional commission to the federal human resource sector was the mandate to sponsor research and evaluation studies with social policy implications. The recent financial traumas experienced by the National Institute of Education clearly indicate that Congress is requiring federal agencies to explicate their social relevancy (Holcomb, 1974). Etzioni (1971) emphasized that policy research, in contrast to basic research, is:

> *much less abstract, much more closely tied to particular actions to be undertaken or avoided. While basic research aims chiefly to uncover truth, policy research seeks to aid in the solution of fundamental problems and in the advancement of major programs.*

The second clear responsibility of federal agencies is to provide timely input for policy makers. Evaluative data presented after a policy decision has been made can have little impact on the decision. Again, Etzioni (1971) has emphasized the difference between basic research enterprises and research conducted in the federal sector:

> *For the basic researcher, science is an open enterprise. There are no intrinsic reasons for the completion of a study at any particular deadline, and the dictum "until proven otherwise" is always at least implied. For the policy maker, there are specified times when fundamental decisions will be made and the decisions made then will become the basis for more detailed decisions. The policy researcher must schedule his research so as to produce conclusions by that point. . .For the basic researcher to conclude that the data at hand are too thin to warrant conclusions is both fully legitimate and in line with self interest. . .For the policy researcher to reach such a conclusion unless the data are extremely poor, is an abrogation of his responsibility.*

Lastly, it is clear that, within the federal structure, programs operate in constant competition with each other, particularly for fiscal resources. Contract evaluation efforts are often initiated to generate data for making decisions about the relative merit of competing programs. With limited fiscal resources available to address a virtually unlimited array of social concerns, policy makers expect agencies to provide evaluative data on program effectiveness for use in making decisions to maximize returns on federal investments.

Charged with these responsibilities, federal agencies, who have considerable latitude and flexibility in this area, then seek evaluation efforts and strategies consistent with these mandates.

The pleas by Everhart (1975) for modifications in traditional federal operating practices are reasonable only to the extent they don't violate these responsibilities. I suspect that major modifications will probably not occur; this may mean that if the relationship between anthropologists and federal agencies is to continue, modifications will be required in some traditional anthropological research techniques.

Critics suggest that the responsibilities of federal agencies are intrinsically incongruent with the anthropological research mode. Others suggest that compromise and accommodation will salvage the relationship and produce a productive partnership. Before any modifications are made, either by federal officials or by anthropologists, the degree of congruence between the federal agencies' responsibilities and the principles, premises, and traditions of anthropology needs to be examined.

Congruence Between Federal Responsibilities and Educational Anthropologists

The federal responsibility to sponsor research and development efforts with social policy implications has led to an increased reliance on awarding contracts. In striking contrast to the grant system, the federal government defines the problem to be explored, not the

researcher. For social scientists accustomed to doing research in academic settings where scholars are given wide-ranging autonomy in defining research problems, this strategy is often a "hard pill to swallow." Since most, if not all, of the anthropologists now participating in educational contract evaluations are former academics, using the phraseology of Bernstein and Freeman (1975), it may be difficult for them to accept this *modus operandi.*

Furthermore, this funding pattern may indeed be in basic conflict with the traditional anthropological approach to research. Everhart (1975) has amply characterized the conflict as the difference between the "making" and the "taking" of the problem. That is, it is generally accepted that anthropologists enter the field void of pre-selected problems and, therefore, "make" the problem as it emerges in the field. Jackson (1974) noted that a distinctive feature of anthropological research is:

> *the absence of formed hypotheses as guides to the inquiry. Typically, the person conducting a naturalistic study does not start with an elaborate theory from which he has deduced hypotheses that are then to be tested. Instead, at least in the early stages of his work, he tends to meander, looking about the school setting with a naive eye, letting the natural flux of events guide his vision. In short, he follows his nose.*

The federal government's attraction to the contract system was, in part, a step toward increased accountability. That is, federal agencies wanted the ability to specify where the researcher's nose should go before the research started. The contract allegedly increased the probability that the final product would have social policy implications and not be just another esoteric contribution to some abstract discipline's literature. If anthropological fieldwork is most applicable to generating problems, as suggested by Everhart (1975), then one inevitably questions the degree of congruence between that approach and the federal system of contract evaluation.

The second responsibility, timeliness, has been a particularly frustrating experience for anthropologists involved in federal contract evaluations.[4] All evaluation efforts, regardless of their approach, have maximum influence only when they provide data consistent with decision-making deadlines. Yet the task of meeting pre-specified times may be particularly difficult for anthropologists, given their considerable immersion in the field, the type and quantity of data collected, and their typical modes of analyzing data. Indeed, Wax (1971) stated in her book on fieldwork that "it is a horrid but inescapable fact that it usually takes *more* time to organize, write and present material well than it takes to gather."

Federal agencies attempting to comply with the fixed times of policy-makers simply may not have the luxury of waiting until the anthropologist has been able to bring the fieldwork to a close. Agencies place themselves in highly vulnerable positions in the federal climate if they sponsor evaluative research efforts which are unable to provide data under the constraints imposed by policy deadlines. Pragmatists in the research community have been willing to provide less-than-perfect data rather than allow the decision-making process to continue in a factual vacuum. It is still unclear, however, because of the relatively short time anthropologists have been engaged in such research, whether anthropologists can accommodate this fact of life and are willing to compromise the ideal. Federal agencies can modify their traditional adherence to report dates specified far in advance; however, they have very little flexibility to provide open-ended times with no consideration of policy-making requirements.

Finally, it is the responsibility of federal agencies to sponsor evaluative research studies which will help determine the effectiveness of various programs competing for limited fiscal resources. In essence, the entire evaluation network was created to maximize the returns of federal investments. The ability of the anthropological approach to contribute to this responsibility has not yet been demonstrated. Indeed, proponents of field studies have noted that the strength of anthropology's approach is in its problem-generating aspects, not in program verification. Iannaccone (1975) proposed that:

> *The field study by its very nature cannot be adequate for verificational research. Its strength, instead, lies in the way in which its characteristic research process, the reiterative cycle of data collection and analysis throughout the study, results in identifying, clarifying and restating problems often as conceptual hypotheses for future verificational studies.*

It is not clear, at this point, exactly how the problem-generating strength of anthropological and sociological fieldwork can aid in evaluating competing programs. Campbell (in Salasin, 1973) noted the lack of good examples of using qualitative approaches in evaluation research (the two studies he does mention were conducted by a sociologist and a professor of law—neither deals with education). There does not exist an explicit statement of just how the anthropological perspective will contribute to evaluation. Iannaccone (1975) and Nelson and Giannotta (1974) advanced the notions of fieldwork providing "richness" of data, a contextual backdrop for quantitative data, and a field-grounded set of hypotheses to be cross-checked with other evaluative data. All these are creditable proposals for increasing the quality of any research effort, but do they lead to evaluative statements? The traditional anthropological perspective is to describe what is, and

not to make judgments about what should be. Jackson (1974) noted that anthropological studies:

> *do not provide answers as much as they delineate the conceptual framework within which the search for answers must take place. In a sense they are more closely akin to critical treatises than to manuals of style. In short, they reveal in concrete terms what has been done. It is left to the reader to deduce what he, in his setting, must do.*

Jackson would have researchers just present data; i.e., describe the situation and then allow the reader to deduce what must be done. Traditional definitions of evaluation have placed far more responsibility on the evaluative researcher. For example, Alkin (1972) stated that "the evaluator's role requires that he make judgments about the relative worth of various courses of action." The ability of anthropologists to assume this judgmental responsibility within educational evaluations has not been demonstrated. Furthermore, if the responsibility of making judgments about the relative worth of various programs is not congruent with the canons of anthropology, then anthropologists need to seriously reconsider their involvement in federally contracted evaluations.

Summary

The purpose of this paper is not to advocate the use of quantitative versus qualitative methods. Conflicts over the use of "hard" data or "soft" data are irrelevant. Rather, this paper attempts to critically assess the degree of congruence between the anthropological perspective and the responsibilities inherent in a narrowly defined type of research—federally contracted evaluation. The issue, therefore, becomes that of determining the effectiveness of a particular methodological approach (anthropological fieldwork) to a particular type of research (federal contract evaluations). Many years ago, Homans (1949) expressed a very pragmatic perspective on this issue. He said:

> *People who write about methodology often forget that it is a matter of strategy,* not of morals. *They are neither good nor bad methods, but only methods that are more or less effective under* particular circumstances.

The qualitative field approach has repeatedly demonstrated its ability to contribute to our understanding of social phenomenon. Indeed, this method has demonstrated its utility by illuminating the complexities of the educational process through the works of Smith and Keith (1971), Cusick (1973), Barth (1972), Jackson (1968), and Rist (1973). However, Alkin (1972) previously noted that evaluative research may necessitate quite different methodologies and analyses than "those which might be employed if the purpose were understanding the education process *per se.*"

Whether the techniques of anthropological research are applicable to evaluation studies is still undetermined. Wolcott (1975) has taken us a long way in defining, from the fieldworker's perspective, the criteria necessary for creating a milieu in which anthropologists can work. Implicit in Wolcott's criteria, and stated more explicitly by Everhart (1975), is the need for reform of the manner in which federal agencies conduct evaluations. This plea for reform is not new and has been expressed by others (Bernstein and Freeman, 1975; Wholey et al., 1970; Williams, 1971). Reformists must realize, however, that there are certain systemic characteristics that constrain the ability of federal agencies to modify their *modus operandi.* Most notably, because they are wholly dependent on Congress for financial support, the agencies must comply with certain responsibilities mandated by the political matrix in which they exist. To violate or ignore these responsibilities is political suicide and certain destruction.

Federal agencies at present have some latitude to change their operating procedures without violating their mandated responsibilities. These changes should be instituted to facilitate the potential contribution of various methodological techniques. Nevertheless, at the point where federal agencies have made maximum efforts to compromise, it may still be necessary for anthropologists to assess whether their continued involvement in this particular type of research will so compromise the ideals and norms of their discipline as to make it unacceptable.

Notes

1. Portions of this paper were presented at a symposium sponsored by the Council on Anthropology and Education at the 1975 Annual Meetings of the American Anthropological Association, San Francisco, December 1975. I wish to thank Drs. John Singleton, Hal Nelson, and James B. Watson for their insightful comments on a preliminary version of this paper.

2. A distinction between the federal system of awarding contracts (as opposed to grants) is crucial to the theme of this presentation. Bernstein and Freeman (1975) noted that

> *contracts are provided when an agency, either on its own or because of executive or legislative instructions and influence, deems it important to undertake a piece of research. Under the contract system, the general rule is that the agency . . . draws up a set of project specifications which state in varying detail the research they wish to accomplish and thus will support.*

The contract system provides less latitude for change and spontaneity during the course of the evaluation efforts because the research activities have been specified

in the contract. Contracts also require considerably more continuous federal monitoring to insure that the contractor is complying with the terms of the contract and to insure that the goal of the evaluative research as defined by the sponsoring agency will be accomplished. Bernstein and Freeman (1975) described basic differences in the contract system and the grant system, which included (1) the process whereby the research is initiated, (2) the process of reviewing proposals, (3) the monitoring process, and (4) the implementation process.

3. Using the characterization of Bernstein and Freeman (1975), federal agencies found that anthropologists are trained to function (and accustomed to function) as academics and not as entrepreneurs.

4. It should be noted that ethnographic description was the primary mode of anthropological research sponsored by the Experimental Schools Program of the National Institute of Education. Undoubtedly, this biases certain perspectives presented here. For example, Singleton (1975) noted that the use of certain other anthropological research techniques would pose no particular difficulties in relationship to the issue of timeliness.

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ETHNOGRAPHY AND EDUCATIONAL POLICY: LOVE AND MARRIAGE OR STRANGE BEDFELLOWS?[1]

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For the past four years I have lived in, worked in, and studied one school district in the state of Washington. Daily I have interacted with my neighbors, teachers, administrators, and especially with students, a routine which has totally immersed me in a microcosm of American schooling. Working under the auspices of a federally-sponsored Experimental Schools Program (ESP) has made this sojourn worthwhile because I have been able to monitor continually the heartbeat of not only the actors in the local setting but also the mysterious and sometimes elusive pulse of officials in HEW/NIE, who have been carrying out what was once heralded as a major new concept in federal educational policy.

While doing a study of student life in junior high school (as well as performing innumerable other chores that are part of working on an evaluation team), I kept a note file on what the experiences of fieldworkers evaluating an educational program reveal about the relationship between fieldwork and educational policy. All the while, I have been waiting for the excuse to compose these thoughts: when I was asked of my interest in presenting a paper on this topic, I was, of course, delighted to accept.

My paper addresses the relationship between ethnography and educational policy. I have chosen that topic for a very straightforward reason: these are the areas closest to my work.[2] The applicability of the paper could, I suspect, just as easily pertain to ethnology, life histories, case studies, or ethnosemantics as to medical, mental health, environmental, or transportation policy. The specific applicability, be it some unit of a culture or a variety of institutional settings, is not important. What is important are the techniques of anthropological and sociological fieldwork as they have applicability to and inform major issues of public policy.

In addressing the wider issue of the interface between policy and anthropological fieldwork, we have to admit that the direct contribution of fieldwork enterprises to policy making has been spotty, indeed, though notable exceptions exist. The Cornell-Vicos project, begun in the early 1950s under the direction of the late Allan Holmberg, attempted to apply knowledge about community change and modernization to development work in a previously impoverished area of Peru. The even earlier work of Alexander Leighton and others in the Office of War Information added valuable information about prosecution of the war with the Japanese and helped determine surrender and occupation terms. Of course, many anthropologists and sociologists do fieldwork related to policy issues, or which could be applied to public policy, but the direct utilization of such research to shape or reform public policy is, unfortunately, too rare. At best, we have the making of policy with an *ex post facto* rationalization for it tied to a selective search of relevant literature; at worst, we have policy being made with no direct effort to use anthropology (or any other discipline, for that matter) to inform the policy-makers.

I propose to explore why so little of what ethnographers have done is used in making educational policy. In order to discuss this topic, I think it would be first useful to review some major characteristics of ethnography as they apply to policy issues and policy formulation. Having discussed these points, I'll next review ethnography and its utility from the perspective of how public (educational) policy is made. I'll also discuss the defensibility (or lack thereof) of a very pessimistic picture of what ethnography can offer. Much as we'd like to think otherwise, ethnographers aren't always the guys in white hats, and I'll point out the responsibilities they must consider if their work is to have an impact beyond that of the readers of esoteric journals. Finally, I'll offer some thoughts on reconciliation.

Some Critical Elements of the Ethnographic Approach

Numerous elements are pertinent to conducting an ethnography in any setting, be it remote Java or the local school around the corner. These elements tend to differentiate ethnography from other modes of research. The first of these distinguishing elements revolves around the definition of the problem to be investigated. Not all ethnographers are anxious to outline in programmatic fashion their "research design," hypotheses to be tested, or samples to be drawn, nor do all of them conceptualize their study by the standard scientific hierarchy of theoretical, conceptual, and operational issues. This is not due to laziness or lack of adequate training in graduate school, but rather to the simple fact that ethnographers, unlike most sociologists, political scientists, or psychologists, prefer to decide after rather than before the fact which researchable problem war-

rants investigation in a particular setting. In this sense, it is difficult for ethnographers to enter any one cultural setting and say in advance what they are going to examine, or that they will examine only sibling rivalry of the Balinese or the effect of modernized curriculum on secondary school children in America. They find it equally impossible to predict that they will use a certain battery of interview questions or projective techniques to get at this information, and that following the collection of these data they will leave the research site and begin analysis. Delineation before the fact presents difficulties because it makes the assumption that the problem stated *a priori* in the fieldworker's (or funding agency's) mind is the problem which most clearly and definitively describes and defines the essence of any one cultural setting.

Problems arise when fieldworkers interact with others not accustomed to such an open-ended approach. My own work occurred under the aegis of a federal project supporting "individualized instruction" in a school district. As I was doing fieldwork in a junior high school, project officials continually pressed me for answers to the question of "how well is individualized instruction operating?" My initial response was to state that my problem for investigation was "what is individualized instruction as students experience it?" As I continued my fieldwork, I soon saw even that question as too focused and substituted instead the question, "what is the process of schooling as students experience it?"

All this is not to say that ethnographers go into a setting without some "foreshadowed problems" to examine, nor does it say that they do not carry with them some conceptual baggage from their discipline. There is no such thing as a *tabula rasa*. It does mean, on the other hand, that ethnographers "make" rather than "take" the problem and that they enter the scene, as Turnbull (1972) says of his own work, with neither

> *specific hopes nor any specific fears, and this was as it should be. It is too easy to go into a field situation expecting or hoping to find this or that, for invariably you come out having found what you wanted. Selectivity can do great things in blinding one to a wider reality. I was interested rather in a very general comparison between two hunting and gathering societies (Pygmies and Ik) in totally different environments; it was more a fact finding mission than the testing of some theoretical point of view; . . .*

Part of the reason for ethnographers deriving the research problem from the field relates to a second characteristic of an ethnographic approach, that of scope. Individuals using an ethnographic approach attempt to cast a wide net as they piece together the complex interweaving of people, events, conditions, and meanings interacting in a specific setting or sub-culture. They do this because of their belief that, just as life itself is a complex gestalt shaped and formed from a variety of pressures acting in myriad ways, so must the investigation of any research problem ascertain the degree to which wide-ranging factors come to bear on that problem.

Again, I found the necessity to trace relationships and to contrast perspectives important in my work in the school district. Working on an ethnographic approach to provide an account of junior high students, I was first satisfied with describing their life and perspectives in school but soon found I needed to account for the place of school in their everyday life. Then I realized that one cannot understand the students' views of schooling unless one understands the view of others in one's role set—the teachers and administrators. I considered including parental life histories but found that too time-consuming to carry out.

Attaining an expansive rather than a restrictive scope is naturally tied to the issue of problem definition. An expansive scope prevents isolating variables for intensive analysis; instead, it pre-supposes that ethnographers will investigate the degree to which actors are influential in a tapestry being weaved. The research problem will then, in all probability, be described and analyzed as a complex rather than a simple issue, a point which leads up to the third and last characteristic of the ethnographic account.

An ethnographic account is meant to be a detailed and rich chronicle of a given setting or sub-culture. In this sense, it must describe enough to provide outside readers with a sense of what it is like to be a member of the group being studied. Indeed, the information should be rich enough and complex enough so that the reader could act out a role in the setting being described—not unlike taking a script for a play and learning the lines, cues, and movements. Ethnographers, therefore, must write what actually happened and how people actually perceived their larger environment, despite the fact that such detail may appear, at first blush, to be excessive and commonplace. Ethnographers must describe the commonplace as it happens in the daily lives of people, and then expand that description by pointing out the significance of what has been described. Their task, as Richardson (1975) has so eloquently stated, is to tell about people in the manner of an epic poet:

> *As teller of the human story, the anthropologist cannot falsify what we are. He seeks to find the full range of human variation, the cruelty, the magnificence, the love that is in us all and in all of our cultures. But the anthropologist is not a passive recorder of human data; he searches for the human secret.*

Such detail and drama cannot be transmitted abstractly or in summary form but rather must convey, as best as possible, the affect of the human condition.

I will have occasion in the material below to return to this discussion of an ethnographic account's characteristics.

What Policy Makers Need and What Ethnography Provides

Until recently, the subject of how policy research should be conducted has been a topic talked around but seldom frontally addressed. Three years ago, however, James Coleman (1973) listed what he called "Ten Principles of Policy Research." Among the points which Coleman made are: (1) That policy variables are those which are subject to policy manipulation. Those which are not manipulable must be treated differently; (2) Policy research should be defined within policy guidelines rather than being defined by the investigator himself; and (3) The ultimate product of any policy research must be a social policy modified by research results. If these are fair characterizations of the criteria for policy research, then we can envision a scenario depicting the ideal type and process of policy research such as:

A government agency is to make a choice on whether to commit funds to educational option "A," which is a funding of schools through a system of categorical grants, or option "B," which is a system of money disbursed for general purposes with schools determining how to spend it. The government calls for individuals and agencies to devise a research design meant to test these options in a variety of settings. The research team concludes that while controls are tighter in the case of option "A," expenses are reduced in option "B" due to fewer administrative costs. Also, recipients approve of option "B" because it gives them greater discretion to use the money as influenced by local conditions. These results are reported to the government agency. Option "B" is ultimately chosen because it seems acceptable by school administrators as a practical and sensible way of having some say about how money should be spent. It is also chosen because it avoids the expansion of federal agencies to process and monitor grant applications and contracts.

A similar scenario for "good" policy research could be developed by following the guidelines described by Mulhauser (1975). In an article dealing specifically with the applicability of ethnography to the domain of educational policy, Mulhauser outlines four basic "givens" of the policy-making process which ethnographers need to understand as they apply their work to current social issues. First, he echoes Coleman's concern that what policy makers need from any research is the "immediate identification of politically viable levers of action." He goes on to point out that the policy maker is usually looking for information on some "mundane comparisons among a restricted set of instruments" and not broad relationships. The former are seen as "manipulable," the latter are not. Second, he points out that this stance is realistic in that legislators and administrators can select only within a small range of choices. In this sense, data are needed on the effectiveness of choices within these constraints although, as he goes on to admit, these data may by no means speak to the "most important problem" or are the key to any complex set of problems.

In discussing why this is the case, Mulhauser sheds some light on the intricacies of making public policy and notes some constraints on its operation. His third "given" is that legislative bills are not pieces of "national life," but rather are discrete efforts which parallel lines of committee, agency, and administrative jurisdiction and turf, a point reaffirmed by Merrow (1974). If this is the case, then the policy process will not be concerned with far-reaching efforts but rather with the availability of information to illuminate the treacherous path of narrow legislative acts as they proceed through the committees of Congress. A fourth "given" centers on the sheer number of players who are involved in the federal educational policy process. There are multitudinous levels through which any piece of legislation must pass and, as Mulhauser points out, "no actor or organization really 'owns' a problem but must negotiate from the outset the definition of the problem and the relevant actors to help in its solution."

Returning to the short scenario presented earlier, the conditions mentioned by both Coleman and Mulhauser seem, then, to have been met. First, policy makers rather than researchers defined the problem to be investigated. Second, the problem as defined fits into what could be considered "manipulative variables," that is, factors that could reasonably be expected to lead to policy action. Finally, we may assume that information on these conditions is such that all actors in the policy process could be expected to take a defensible and reasonable position on it to insure approval. Such choices within a relatively narrow range permit the art of compromise with neither the surrender of critical values nor the necessity to assume a radical position.

Research on the subject of choice between one of two funding options would, in Coleman's and Mulhauser's terms, be considered good policy research, and given their terms there is no doubt that it fits the need to implement public policy. It ignores, however, the consideration of any information to inform public policy—to provide policy with a different direction or to force it to consider the roots of educational issues rather than just the tips. Thus, the principles for policy research as stated by Coleman and Mulhauser are both narrow and parochial. What if option "B" is not more expeditious than option "A"? What if neither of them is found to make one bit of difference in anything that makes a difference? So monies are funded from the

federal level directly to the schools rather than by way of a specific and definable program: how does this fit into the entire climate by which school districts make decisions, run schools, educate students? How does it relate to the ongoing dynamics of the administration and organization of American schools? The type of policy research we have been discussing will tell us little about those issues whereas an ethnographic study of a district central office could not only inform policy makers about funding options but, more importantly, describe the culture of administrative offices and the role of fiscal management within that culture.

I can speak from experience on the subject of whether policy research is broad or programmatic because some of my colleagues and I spent considerable time evaluating a federal education project on this very issue. Receiving severe criticism from our own parent organization for not looking solely at "outcome variables" and for not basing our entire five-year effort on how every action had implications for project objectives, we argued that the project had to be examined in its context, that is, as a "transplant" of sorts into the living organism of the school district. This "illuminative" approach (Weiss, 1966; Weiss and Rein, 1970) focuses on how things work rather than simply how well they are working. We attempted to examine a school district holistically, not only in terms of its place in the community and the everyday activities of school personnel and clients but also by identifying forces responsible for moving the program in a direction. "Program objectives" and "outcomes"—aspects ostensibly crucial to good policy research—were found to have little importance in the lives of most school personnel when placed in the context of their daily lives and the regularities of schooling and the community.

But we can see that any attempt to examine a setting holistically and, more specifically, to conduct an ethnographic approach on some aspect of that setting, creates a disjuncture between the precepts of ethnographic studies and the ostensible conditions under which such studies are said to be "useful" for the making of public policy. Earlier, I described what I saw as some characteristics of an ethnographic approach and I outlined those characteristics in terms of problem definition, scope, and detail. In terms of policy studies, then, an ethnographic approach runs into considerable difficulty. First, the problem for investigation is frequently altered or even determined after the fieldworker has been on the site for some period of time. While it may be legitimate for policy makers to guide the ethnographer in certain directions (such as examining the educational processes in a given community and schooling in that context), few ethnographers would find it tenable to conduct fieldwork where they were forced to study "alternate ways to tax and subsidize, to regulate organizations and individuals, and to channel funds through one or another set of administrators at federal, state, and local levels" (Mulhauser, 1975)—issues supposedly crucial for educational policy.

The second place ethnography runs into roadblocks in the area of public policy deals with the related area of scope. Not only do ethnographers prefer to pursue their definition of the critical problem but, once defined, they want to investigate the broad parameters of that problem. My own work on junior high youth has followed in that direction, although it certainly is not as holistic as that which might be done by some colleagues who are more anthropologically oriented than I am. But my work is considered by some as "irrelevant" because it doesn't focus directly on project goals and objectives. My response is that while such objectives mean something to technocrats and policy officials themselves, they have less meaning in the ongoing experiences of junior high students or even their teachers.

Finally, ethnographic work tends to be dense and rich, posing a critical problem for policy makers who don't have time to read such voluminous material. As we have seen, policy work needs to be crisp and to the point. The color and emotions portrayed in an ethnographic account are seen as excess baggage that gets in the way of "the facts."

In reviewing what policy makers say they need from the world of research and what it is ethnography provides, there is an obvious disparity or "mismatch" (Mulhauser, 1975). Policy makers want quick and simple information on a focused problem in order to provide information on variables that can survive the administrative-legislative process. Ethnographers provide broad and dense studies on areas which may or may not be considered to be policy issues. In looking at criticisms of ethnography and its effect on the making of public policy, I am struck by the emphasis on expedience and pragmatics adopted by those in policy circles. This is certainly understandable, for in the world of action and getting things done, long-term gains are usually surrendered and compromised for short-term effects.

Yet, I have little sympathy with the criticism that ethnographies do not define some readily identifiable lever which a policy maker can pull in order to change a social program. What many policy makers fail to recognize is that policy itself is a cultural phenomenon, subject to standards and values that are constantly changing and which are meant to be altered to fit a variety of circumstances. George Herbert Mead said that men create their own world and their own view of that world; so do they come to form and to accept various views on what constitutes a lever for social action. Public policy then is not cast in concrete, molded by policy makers who have objectively determined where policy changes can be affected and where they cannot. Public policy is a political process, and the political process is as much symbolic and ceremonial as it is a tightly

rationalistic system. Because of the non-linear dimensions of policy making, policy is meant to be pushed and expanded, informed and debated. In this sense, it need not be a "minor variation" dealing with some relatively "mundane problem," but can and should be expansive and illuminative. In our own country, for example, we have come to accept a five to six percent rate of unemployment as permissible, while in Scandanavian countries such a rate would be the cause of swift policy action. In this country, we have come to tolerate an educational system which allows a full twenty percent of the population to be unable to exhibit any modicum of basic survival skills, yet we ignore many factors which ethnographic approaches, such as those by Cusick (1973) and Rist (1973), identify as contributing to that condition. Those factors are said to be "not manipulable."[3]

Thus, the critiques of ethnographies raised by many policy makers are also critiques of their own lack of vision on what the parameters of public policy are and could be. Both Coleman and Mulhauser seem content with attacking all policy research that does not fit a narrow, pragmatic, and utilitarian model. They want policy research to predefine the problems, to deal with politically expedient variables, and to be brief; everything else is peripheral. Their obsession with pragmatism and the status quo explains the policy makers' call for ethnography to adjust to the practical demands of the policy process, while they refuse at the same time to examine the very process by which policy is made.

The Needed Contribution of Ethnography to Educational Policy

Thus far, I have discussed how educational policy is made and some criticisms ethnography and its use in making educational policy. It is clear that I have little sympathy with some of these criticisms, and I have faulted policy makers for making the assumption that policy is as it is and demanding that ethnography adapt to its standards.

Yet, I think if we look in the mirror, we see a reflection of ourselves which is not as bright and uncorrupted as we would like to believe. Indeed, in this section, I'd like to maintain that ethnographers are not the knights in shining armor that they think they are (Kimball, 1975), and that they, too, need to examine carefully their own work vis-a-vis the formulation of educational policy. I should note here that many ethnographers do not feel that their work should have policy implications, and I would not be so daring to suggest that all should; I do, however, claim that many more could.

One limitation of many ethnographies is that they fail to tie the rich and case-specific data to any developmental literature that could add considerable insight to the case being described. Certainly, they often cite other anthropological literature to support (and sometimes refute) a variety of points being made, but this is not the type of developmental approach of which I am speaking. Instead, I see that ethnographers are either unwilling or unable to consult and digest the variety of literature existing outside of (as well as within) their own field and bring it to bear on the specific phenomenon they have examined.

Let me offer a case in point. The series, "Case Studies in Education and Culture," edited by George and Louise Spindler and initiated almost 10 years ago, contains some very interesting and readable ethnographies dealing with education in a variety of settings. I have read many of the books in this series and find them commendable. But they lack any systematic attempt to tie the findings to literature so that those in policy circles could take the findings, compare them to similar findings, and arrive at an informed judgment about how policy may be reconceptualized to account for those findings. Most of the books in this series deal, in one way or another, with such topics as acculturation, the nexus between the community and the school, education and the economic order, the school in its political context, the relationship between the school and the state, classroom dynamics, or modernization. Yet seldom do these ethnographies of education attempt to inform the reader on what studies in not only anthropology but political science, sociology, economics, history, indeed, even literature, have to say about the findings and interpretations reached. At best, these ethnographies treat us to a short, one- or two-page conclusion which informs us in some general way that the school is part of the larger society in which it is imbedded.

Of course, the studies in the Spindler series were not originally conceived to have policy implications, so it is difficult to fault them for a sin of omission. On the other hand, is there any reason why ethnographers could not incorporate a wide body of findings from any number of fields to better place the study in some more general and broad perspective? This approach—a holistic mode of analysis to complement the holistic nature of the fieldwork—could make ethnography more influential in informing significant educational issues.

A second limitation of ethnographies in their relevance to public educational policy is the ethnographer's penchant for a functional approach to describing social phenomenon. I don't see this as a problem unique to ethnographies but they share the criticism with any number of research modes and thus must be held to some of the same limitations.

I don't want to indict structural functionalism as an inappropriate way to look at some social phenomenon, for I feel it does describe a great deal. Yet, a functional approach is often used simply to describe why a given social or cultural setting maintains itself in the manner

which it does, with little consideration for patterned inequities, institutional power, ideologies, or the internal dynamics of how a system works and for whom the system is not functional. Sociology journals were filled with such criticisms and rejoinders a number of years ago. Davis and Moore (1945) posited a functional model of social differentiation to explain how society allocated rewards and distinguished among its members. Critics, on the other hand, were quick to point out how a system of stratification, as analyzed by Davis and Moore, had its dysfunctions as well. In the same vein, functional analysis places schools into a systemic equilibrium and shows how they are part and parcel of the entire social system. Functional analysis too often ends there, with little consideration for or opinion on whether such an equilibrium (if it exists) is desirable and how it might be altered, for whom, and at what cost.

I think that ethnographers, although they may find functional analysis to be useful at times, should attempt to either use it more creatively (Gans, 1972) or be encouraged to use other modes of analysis. There is a tendency, when dealing with systems and analyzing a setting holistically, to fall back on functionalism as the most obvious mode of analysis. But this need not always be the case and there is nothing inherent in any examination of education and its context st[illegible] that a functional approach must be used. The work of Jules Henry was anything but a functional analysis; some recent work by British sociologists of education using a sociology of knowledge perspective (Keddie, 1971) suggests the uses of an ethnographic approach in other than functional analysis. The point then is, and should be, that ethnographers have a variety of leverage points so they can describe not only how a system fits together, but how it does not. I think ethnographers could examine social phenomenon in terms of dissensus as well as consensus, thus offering a more specific and particular point of concern for policy consideration.

Another limitation of the ethnographic approach and its goodness of fit with policy research is its failure to consider how patterns described and perspectives recounted could have any relationship to larger order questions involving public policy. This point is related to the first point on the incorporation of relevant findings in other fields but some additional foci are still in order. One focus centers on what is a failure to apply ethnography to policy issues, the other on what seems to be a fear of applying an ethnography to policy issues.

Unfortunately, too many ethnographers fail to take ethnographic studies to the next step to be able to answer in a detailed fashion the "so what" question. Perhaps this is and has been all part of the "scholarly tradition" wherein academicians have found it more comfortable to talk to each other rather than place their insights on the line before the general public, thereby being stripped of the jargon and sometimes pretentious analysis behind which they stand. I can sympathize with this feeling, for indeed I have found my most difficult and embarrassing moments to be not when I present a scholarly paper before my peers but when I attempt to describe what I do to my neighbor, who is a member of the Teamsters Union.

I think this is too bad. If we can't translate our fieldwork findings into anything other than such gobbledygook statements as "the most basic problem that arises in connection with knowledge utilization may be those that stem from the social and organizational character of educational institutions," then we can blame no one but ourselves for the fact that our voluminous ethnographic studies are not highly regarded by many outside our own little club. In general, and where appropriate, there is no reason why studies on socialization or acculturation patterns cannot address issues of public policy, or why studies of community values and receptivity to change cannot be equally applied, or why ethnographic studies of student culture cannot be applied to the entire subject of learning and cultural transmission in a particular society. They can be but aren't, and I sometimes wonder if we as sociologists or anthropologists are so limited in our perspective that we simply don't know how to consider the import of our work beyond the debate of theoretical frameworks.

Some ethnographers exhibit a fear or hesitancy to take a stand and state how they, the ethnographers, see the particular study relating to a policy issue. The argument is often advanced that description, and description alone, is what ethnography is all about and it is not the ethnographer's responsibility to offer any substantive comments on what certain data mean in terms of judgments about what could or should be done differently, nor are they capable of doing so. I suspect this stance is rooted in the notion of cultural relativism and the accompanying belief that the ethnographer's job is only to say how a system does function, not how it might operate. Some have even argued that a statement of judgment on how something should work precludes objectivity in the study of human behavior and that those who feel compelled to "call shots" are to be accorded less than full membership in the club (Wolcott, 1975b).

I personally find this to be an unrealistic assessment of not only how ethnography is conducted but how the human mind functions. To believe that by not stating opinions or making judgments somehow purifies the data that have come before and makes them "more objective" is naive. We all go into any situation with our own preconceptions and our own biases. Ethnographic description itself is biased, for the ethnographer has had to make decisions about what events to portray and which to leave out, what to emphasize and not to. By pretending that these factors are not there makes the description no more "valid" than by stating one's

framework beforehand and then attempting to compensate for that bias as much as is possible. Similarly, making judgments and calling for policy recommendations after dispassionate description makes a study no less ethnographic, anthropological, or pure than does merely providing description alone. I don't think that the involvement of ethnographers in the making of educational policy necessarily corrupts the discipline and makes it less respectable or scientific. As Hymes says (1972), "One should react to the utterance of 'that's not anthropology' as one would to an omen of intellectual death. For that is what it is."

Towards Reconciliation

The question we now face is whether the application of ethnographic approaches to the making of educational policy is possible or whether there will always be an estrangement.[4] While we may expect some degree of reconciliation, I think it would be unreasonable to expect a blissful marriage. Like it or not, policy is usually not made using the information policy makers have about the merits of one approach or the other but on the bargaining and compromising between individuals representing various interests and interest groups. As Redman (1973) has so vividly portrayed, policy legislation is a dance which responds to many cues and which moves in many directions before it is completed. Such is no less the case when policy is made by administrative rather than legislative decision. If this is true, it is not only ethnographic data which are virtually unused (and unusable) but most scientific data as well. The political process often precludes the use of "scientifically rational" data in much of its operation. If such were not the case, then how could the findings of the National Commission on the Causes and Prevention of Violence and other similar bodies go unheeded (Platt, 1971; Komarovsky, 1975)?

Given these limitations, I think there are certain responsibilities both ethnographers and policy makers have if there is a potential for reconciliation and if, indeed, more definitive information is to be used as the basis for educational policy. Some have claimed that ethnographers, indeed, social scientists in general, have no place in the making of public policy. Moynihan (1968), for example, criticizes the pretentiousness of a small group of sociologists interested in problems of deviance for giving the impression that they had *the* answer, for convincing the government to adopt their "answer," and then watching the whole effort fail. Nisbet (1975) accuses social scientists in general of "giving and demanding the right to give advice as giants while we were still pygmies." He goes on to say that social science in general should stay out of public policy, stating that the "purpose of science is to search for truth—not to advise governments, save mankind, make public policy, or build empires." Science, in Nisbet's view, should remain in the cloistered halls of the campus and scientists should speak to themselves.

I don't think that ethnographers or any social scientists have to quit dealing with policy issues but I do think they have to do a better job at it. Certainly, we have to recognize that our knowledge is fragmented and somewhat imperfect but as Coleman (1973) himself has said, "Partial information at the time an action must be taken is better than complete information after that time." Ethnographers must be willing to grapple with the hard realities of the imperfection and generality of their information but this should not dissuade them from making informed judgments after having presented a description as accurately and as objectively as possible. We should not lead people to think we have the forces of prediction in our grasp but we shouldn't be afraid to venture a probability.

In this respect, ethnography, like any other science or art, for that matter, might be akin to Stretton's (1969) analogy of the practice of law, where anthropologists and sociologists describe, explain, and even interpret behavior "roughly but well within local limits, and inventively when required." He goes on:

> *the Law has principles, no Grand Theory. Its principles are moral, political, admonitory. But the general principles themselves are rarely reliable for deduction, and they clearly never should be. Lawyers do not dream that a few parsimonious, overarching laws may one day subsume all others. They know the difference between Locke's subject matter and Newton's. They neither suppose their science to be young, nor sit industriously under apple trees.*

If ethnographers on the one hand are to be more willing to suggest what their descriptions mean and how they apply, then policy makers on the other hand must be willing to open some doors and allow ethnographic approaches to become working tools for the informing of educational policy. Coward (1975) states that "because the agencies are wholly dependent on Congress for financial support, the agencies must comply with certain responsibility parameters mandated by the political matrix in which they exist." Dreyfus (1976) argues that Congress is limited to summary action rather than to the implementation of major policy directives, a role reserved to the executive branch. Yet this line of reasoning suggests that the federal system of policy making and policy implementation is a given and somehow cannot be altered. It also disguises the fact that there is enormous discretion in the political process, and that in the area of education many programs supposedly "mandated" by Congress are selectively enforced and implemented by government agencies (Murphy, 1971).

Unfortunately, I think that the refusal of policy

makers to look at their own side of the fence as well as the other side indicates a lack of vision and a too great willingness to accept what is rather than to speak out for what could or should be. We need a greater sense of intellectual climate in the area of public policy—not just a search for answers but, as Richard Hofstadter once said, the ability "to turn answers into questions." In this respect, policy has to become less concerned with pragmatics and more concerned with the problem at hand.

Finally, and related to an earlier point, I think we need to make the distinction between ethnography as a way of assessing the effects of a program linked to a policy and its use in assessing the very policy itself. Clearly, I think, ethnography—and possibly applied research in general—is more suited for assessing a policy and is less valuable as a way of assessing only the program, which is part of the policy. In this respect, ethnographers can and should be willing to relate their findings to the basic premises and objectives of a particular policy. In the same light, policy makers and implementers should welcome this contribution because it adds to what Cohen and Garet (1975) have termed a "discourse about social reality—a debate about social problems and their solutions." This is, I think, what we need more of—a forum for discussion, not just on answers but on the questions to which proposals are the answers. The commitment of the ethnographer to context, to description, to the meaning of everyday life can contribute immeasurably to that discourse. The commitment of the ethnographer to enter the discourse and the commitment of both parties to be informed by the discourse can help remove from policy research the stigma which Henry Brooks Adams once laid on philosophy—"unintelligible answers to insoluble problems."

Notes

1. This paper was presented as part of the symposium, "The Wider Applicability of Anthropological Methodology," at the meetings of the American Association for the Advancement of Science, Boston, 24 February 1976. The paper benefited from the criticisms of Raymond Coward, Wayne Doyle, Toby Edson, William Firestone, Don Messerschmidt, and Harry Wolcott.

2. At the outset, let me say that my training has been in sociology and education, not anthropology. I have long had an aversion to the preoccupation of most sociologists with social structure and survey analysis, thus my natural attraction to anthropological fieldwork. Neither do I want to claim that the work I have been doing the past three years is necessarily ethnographic; rather, it is possibly what Wolcott (1975a) has termed an ethnographic approach to research in education.

3. Here is where the ethnographer should be at his best. He should be able to describe an educational practice, examine the assumptions underlying it, and then relate the findings to a broad body of comparative data. He should be able to outline the cultural assumptions in both policy formation and implementation and then show, if necessary, that these assumptions may be changeable if viewed in the proper perspective.

4. Some reasons for this estrangement are outlined in Merton's (1968) discussion on the role of the intellectual in public bureaucracies.

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ON THE ETHNOGRAPHIC PROCESS IN ANTHROPOLOGY AND EDUCATION[1]

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Anthropology and education as a field of inquiry has inherited many things of value from anthropology. Like the wider discipline of anthropology, educational anthropology has a cross-cultural or comparative framework, it places a high value on context and situational variables, and for the most part, it demands that knowledge about schools and education be gained through intensive field study. But anthropology and education has also inherited some traits of anthropology that may not be quite so powerful for understanding education. One such trait is a lack of thought about what makes up an anthropological study of education. While it is common to read about the techniques and activities of fieldwork, participant observation, and the dilemmas of being a stranger and a friend to the subjects of research, little has been written about research design and the writing up of a study. In anthropology, this narrow range of topics in the developing "self-consciousness" (Nash and Wintrob, 1972) of the discipline has a historical base. As ethnographers left to study exotic cultures in far-off lands, it was necessary only to record "as much as possible" about the unstudied society. Such an approach was beneficial in that anthropology developed as the most general of the social sciences and the diversity of the human condition became the hallmark of the field. In the old days, anthropologists were sent out on "expeditions" to the frontiers of the planet or, more often, sent to a small community by themselves and told to "learn the culture."

Times have changed. Today the problems of selecting a field site where a project can be carried out involve the willingness of the host society to be studied, the availability of funds, and the applicability of a theory or hypothesis to the location. There is need to be more explicit about the kind of knowledge to be gained from a particular study. Serious thought has to be given to the format, audiences, and utility of study findings. Unfortunately, educational anthropology has continued to follow the parent discipline of anthropology in ignoring these topics in favor of the more exciting aspects of doing the fieldwork. But people are not transported into a school system like the crew of the *Starship Enterprise;*[2] studies, films and other products of research do not magically appear at the close of a study. It is my purpose here to take a comprehensive look at research in anthropology and education. I will not be reviewing the various models of research design implicit or explicit in the literature of the field, but rather will propose a model or heuristic paradigm for an anthropological study of education. In order to exemplify the content of this model, I will draw upon a study I have been doing with Abt Associates of the schools and community of Willcox, Arizona. But the main point of this discussion is the structure of the enterprise. The whole of an anthropological study–from deciding who, what, and where to study to publicizing results–can be termed an "ethnographic process." Within this process, four major components can be identified: research conditions,

research design, research implementation, and product creation. At first glance, these four components appear to make up a temporal sequence; indeed, the limitations of prose support this appearance. But in real life, especially the life of a field study, these four components are in constant motion. The conditions of research change, as does a study design and its implementation. Products are assembled at the end of a study and perhaps edited or rewritten but their creation is a continuing activity.

Before I turn to a discussion of each component of the process, I will first examine some of the dangers inherent in ignoring this process, dangers which can limit the contribution anthropology can make to educational policy, theory, and practice. Following this, I will outline some of the barriers that have kept people from looking at educational anthropology in a comprehensive light.

Dangers of Ignoring the Ethnographic Process

The basic danger facing educational anthropology should the whole of an anthropological study be ignored is the conceptual condensation of the discipline into the idea of fieldwork. Doing fieldwork or participant observation *is not* anthropology. Because of the inordinate attention usually given to fieldwork in the literature, educational anthropology is in danger of being equated with it. It is important to note that the relationship of fieldwork to educational anthropology is not reciprocal. While it is difficult to imagine an anthropological study which does not include fieldwork, it is certainly possible to do fieldwork without doing anthropology.

If the problem were merely the mislabeling of research (how much more exciting a study sounds if it is called "anthropological"), then it would be easy to send out warnings and admonitions to the perimeters of the discipline and hope that the researchers stealing the name and some of the thunder of anthropology would take heed. But the problem runs deeper. There is a danger that those commissioning studies of education by anthropologists will take the false equation as reality. Some signs of this possiblity have already emerged as shown by Clinton's (1975) portrayal of the anthropologist as a "hired hand." If such a trend continues, anthropologists of education may become the "instruments" of the many sister disciplines studying schools and their contexts. To be recognized only for their ability to elicit extensive information from school people in unobtrusive ways, anthropologists would be in danger of becoming personified questionnaires roaming the schools of the world.

An additional danger in equating educational anthropology with fieldwork is that development of other components of the ethnographic process will suffer. If careful attention is not given to research design and the evolution of research implementation, then educational anthropologists will not be recognized as authorities in educational research planning and policy. Educational anthropology will be put in the position of reacting to the proposals and plans of others rather than generating problems and new ways of looking at schooling themselves. Likewise, if the creation of research products is not taken seriously, then it is very likely that the time, the support, and the editing of the products will be relegated to a residual category in a study. Writing up monographs, creating audiovisual materials, and in other ways publicizing research findings, are too important to be left to chance. Wax (1971) has noted that a study's write-up generally takes more time than doing the fieldwork, a point forcibly seconded by Wolcott (1975). But because many educational anthropologists are trained in the tradition of old-time ethnography, which emphasized a separation of fieldwork from the writing of an extensive monograph, writing a report is often thought of as something that occurs after a study. A more prudent approach would be to recognize that an anthropological study of education seldom tells you only one thing about schools; nor does the knowledge gained in a study wait until the end of a period of fieldwork before it becomes visible. The component of report preparation must be considered as a major and continuing part of an ethnographic study. A practical result of ignoring this feature, one that many of us have faced in the field, is finding that for political, economic, or policy reasons support for product preparation is lacking after fieldwork has been completed.

A third danger in not looking at an anthropological study as a comprehensive operation involving several equally-important components is that future students going into the field will have a distorted idea of educational anthropology. There has been a lot of talk lately about the need for preparing graduate students in anthropology (D'Andrade et al., 1975) and education (Eddy, 1976) for the uncertain job markets of the future. While most of these reports stress the need for adaptability among the scholars of the future, it is important to note that any anthropological study of education has the potential for teaching students the diverse skills of the trade. It would seem to be the height of folly to caricature educational anthropology as the "problems of fieldwork" while also attempting to convince new students that they need to adapt to new frontiers.

Anthropology has always been a risky and dangerous business. When my family and I undertook research in Yucatan, Mexico, several years ago, we were warned that tropical disease could easily claim one of our lives. Cutting short a field study because of illness or death is not uncommon in the history of the discipline. Today in educational anthropology new dangers confront students of the field; while they may not be as physically threatening as the ague, they can severely retard the

development of anthropology and education. Ignoring any of the components of an ethnographic study of education can lead to the false impression among policy makers that anthropologists of education "don't know quantitative techniques" and are best "left in the field." At the level of an individual study, the danger in ignoring the complexity of the ethnographic process manifests itself in the lack of time or support for adequate conceptualization, analysis, and writing, or in the inappropriate use of research techniques in the field.

Barriers to Examining the Ethnographic Process

At the outset, I suggested that the discipline of anthropology has shied away from discussion of such things as research design and report preparation for historical reasons, and that educational anthropology has followed suit. This barrier raised in front of a critical examination of the ethnographic process is a barrier built on tradition. Three other barriers have obstructed the dialogue on the process: the dilemma of anthropology as art and as science, the lack of competence among scholars in the field, and the lack of a vocabulary for discussing the issue. The last of these—the lack of vocabulary—is, of course, the topic of this essay. While I make no claim that the particular terms I am using here are the final words on the subject, I do hope that they at least open a dialogue.

Whether anthropology is an art or a science is a question which continues to be posed by practitioners of the discipline. Educational anthropologists are not immune to the implications of the question, although as yet they seem to have been spared some diatribes that have characterized the debate in anthropology. I prefer not to think about anthropology-as-art-or-science as a question with a right and wrong answer but rather as a dilemma. The discipline deals with human beings: as such, it is faced with all of the vagaries, contradictions, and hidden motives that humans have developed over the past few million years. Still, it seems possible or at least worthy of effort to abstract regularities and similarities from the human condition.

In the past, "either/or" rather than "both/and" thinking has characterized discussions about the art and science of anthropology. The editors of the monumental and inspiring *Handbook of Method in Cultural Anthropology* (Naroll and Cohen, 1973), for example, stress that they would like to "see anthropology become a progressively more rigorous and scientific branch of the social sciences in general." Partially in response to this and to other calls for more science in anthropology, Honigmann (1976) recently proposed a return to the "personal approach" of the discipline. This debate over science or art tells more about the way anthropologists think than it does about the study of people in cross-cultural settings. Binary thinking based on polar opposites has long dominated many debates in anthropology, most of them related to the science/art dilemma. Powdermaker (1966) talked about the anthropologist as "stranger" (i.e., scientist) and "friend" (i.e., humanist). Most introductory textbooks make a distinction between "real" behavior (what people are observed to do) and "ideal" behavior (what they say they do). Transformed by some of the ideas and vocabulary of linguistics, this division of the world into real and ideal categories became the basis for a debate over the study of "etic" or "emic" behavior (Berreman, 1966; Harris, 1968). A recent discussion in *Current Anthropology* (Paredes and Hepburn, 1976) has suggested that the "culture and cognition paradox" is related to the different hemispheres of the brain. While it is possible that the right or the left side of the brain is responsible for people being "intuitive" or "analytical," there is growing evidence suggesting that the two halves of the brain work in concert, and "right" or "left" thinking dominance is only a popularized notion (Harnad and Steklis, 1976).

The reason that the science/art dichotomy has served as a barrier to a realistic assessment of the ethnographic process is that the combatants on both sides of the battle-line have made ridiculous demands. Those who hold an "artsy" view of anthropology recoil from the thought of setting down a rational model for what they do. There is a fear that by stating *exactly* what will take place during a study one limits the intuitive, often random, activities that are needed to maximize the anthropological way of knowledge. There is a sense among the more humanist anthropologists that something of the "beauty" of a study will be lost in this way. I am reminded of a remark an ethno-musicologist said to me once about the place of analysis in the discipline: "you anthropologists take apart culture like a biologist takes apart a butterfly. You never see the beauty of its flight."

On the other side of the coin, anthropologists looking for more science in the discipline call for explicit discovery procedures (Tyler, 1969) and careful confirmation of all ethnographic statements (Moerman, 1969). Naroll (1973) has suggested that anthropological knowledge is nothing more than a stochastic probability chain.

The barrier of the science/art dilemma in anthropology needs to be dismantled quickly. Taking the stand that an anthropological study is mysterious and personal, and thus not amenable to rational dialogue, is as bad as asserting that the intuitive and aesthetic aspects of the enterprise be banned from the discipline (Werner and Fenton, 1973). It is time to take heed from the writings of science and art (Ghiselin, 1963) and admit that intuition, inspiration, and plain hard work are characteristic of the search for knowledge. Although it may appear difficult to propose using "insight based on serendipity" in a research proposal, such an attempt still

should be made.

A more down-to-earth barrier has kept the discussion about the ethnographic process limited—the barrier of scholarly competence. Although anthropologists of education would like to see themselves as reincarnations of the proverbial Renaissance men and women; in actual fact, all of the skills involved in the process, including organization, planning, fieldwork, and writing skills, are seldom found under one hat. Some people are very good at designing a research project but not well equipped for putting plans into operation and gathering data. Others may be sensitive fieldworkers but poor writers. Still others may be thoroughgoing researchers and excellent writers but have no sense about how to pick a field site. The folklore of anthropology and education is replete with tales of scholars who have either excelled or fallen on their faces while carrying out a study.

The barriers to examining the ethnographic process as it applies to educational anthropology have not served a useful function. Instead, they have served to channel and limit the exploration of what an anthropological study is into the rut of writing about fieldwork experiences or into the debate of scientism versus humanism. If practitioners of anthropology and education feel at a loss to describe how they write or how they plan a study, it is probably due as much to a lack of attention to these topics as to their ability as ethnographers.

Conditions of the Ethnographic Process

The conditions under which a study is carried out range from the highly personal preferences one has in relationships with other people (some researchers study principals, others study kindergarten students) to the institutional setting of the research. For the purposes of this essay, I will not dwell on the highly personal aspects of research conditions except to note that they, too, are subject to change throughout a project. The disciplinary interests of a researcher, the location of the project under study, and the institutional context of the study will be considered here.

The interests one has developed in the discipline of anthropology of education form the basis for the whole research enterprise. If this essay were in the tradition of outlining the formal steps of theory testing—statement of problem, development of hypothesis, hypothesis testing, and analysis—then the disciplinary interests would belong with a discussion of problem statement. Honigmann (1976) noted that the configuration of factors influencing the choice of what to study often depends on such things as the popularity of a topic or a research technique or the availability of funds for certain kinds of research. It should come as no surprise that academic fads or trends have a great influence on the research marketplace. The importance of Honigmann's observation is that the statement of the problem or the researcher's disciplinary interests are related to other conditions. It is thus necessary to examine disciplinary interests in light of the other conditions of research.

In my study of a small Southwestern community, the disciplinary interests I brought with me included an interest in the ideational systems of ideas that people created in order to get along and effect social change. My view of culture and society was heavily influenced by Wallace's (1961) idea that people had different "mazeways" (or configurations of the way they perceived the world) which worked in a community in a way that made both social order and conflict a fact of life. Seen in terms of the study of a large, comprehensive project of educational change in a small town, my interest in the "cultural maps" that people carried around in their heads was translated into researching how different actors involved with a National Institute of Education Experimental Schools project looked at what they were doing and how the total project appeared to them. Taken one step further, I was interested in how the several versions of the project would affect the way students, teachers, and other community residents either accepted or rejected the kinds of educational changes that were proposed.

A second disciplinary interest was in the place of contextual variables as they related to planned change. One of the basic insights that anthropology has to offer is that people do not always act as their institutional roles might predict but, rather, approach every social exchange with a complex set of expectations and strategies for behavior. In the specific case of Willcox, Arizona, I was interested in the place that a local or regional identity might have in the implementation of a federal change program, and whether such factors as ethnicity, linguistic pluralism, or kinship would influence the way the school system operated in general or the federal project operated in particular.

A third interest, deriving from the discipline of anthropology and education, was in the naturalistic study of schools. This interest bordered on the methodological in that it assumed that long-term observation was necessary to develop a model or description of what happens when a total school district attempts wide-scale innovations. Does educational change follow a step-by-step advancement, or are there early and late changes that don't seem to coincide with an external measure (such as the month or year of implementation)? Is there something that can be identified as "the project" in the district or are there several innovations that become the project *ex post facto?* These kinds of questions could only be answered if sufficient time was given to day-to-day observation of the school district over a long period of time. Although the particular educational change occurring in Willcox had characteristics of change by diffusion, change by "cultural brokers" or entrepreneurs, and change by remote control through the funding agency in Washington, D.C., anthropological

skepticism suggested that an analysis of the change program in the schools had to wait until observation took place.

Other interests that make up the conditions of research are related to the location of the school or community under study. The selection of a research site where the problems one seeks to solve can be reasonably delineated is an extremely important task. Ideally, the locale of a study should hold both intellectual and emotional interest to a researcher—it is too easy to allow the dislike of a community or school to overcome the objective and intensive base of a study. In my study of Willcox, I had little control over the specific community I would be studying; the selection for the rural Experimental Schools program sites was made by the National Institute of Education. As a part of the summative evaluation research team,[4] there were ten possible communities that I might have had the opportunity to study. I was interested in going to Willcox because it was the only community chosen in the Southwest, a region where my family and I wanted to live. In addition, I had completed a socio-linguistic study of several Mayan communities in southern Mexico, so the problems of bilingualism and the influence of Mexico on a community fascinated me. It turned out that Willcox was extremely easy to live in, and our stay there was very rewarding. Studying a community that can be called "home" is a special pleasure.

Willcox, Arizona, is located close to the Mexican border and is diverse in language, history, occupational base, and environment. This diversity was reflected in the school system's proposal for educational change, so the local project provided an excellent test case for studying change designed to make an institution more sensitive to its cultural environment. This particular emphasis in plan was especially amenable to being studied by an anthropological approach which traditionally placed a priority on understanding the influences of environment and culture as applied to social change.

I was hired by Abt Associates, a private, applied research company, to do a case study of educational change in Willcox and to assist in several complementary studies carried out by survey researchers. Such an institutional setting is not that common in anthropology and education, though there is a chance it may be more common in the future. The institutional conditions that had a direct effect on my study included the fact that three major survey studies were being conducted in this relatively small community (5,000 people) while the schools attempted broad changes. In addition, the contractual arrangement between the National Institute of Education, the school system, and Abt Associates made for a confusion of sorts—it was difficult to understand how I could be working in the schools, an employee of the research firm (Abt), but somehow be receiving funds from the government.

The institutional conditions that proved most troublesome were the "research overkill" that local people noticed emanating from the company, the tendency for the survey researchers to use me and other on-site observers to do their legwork in collecting data, and the lack of support and constituency that anthropological studies had both from the company and from the National Institute of Education.

On the other hand, the institutional conditions of Abt and the National Institute of Education made it possible to do a three-year field study of a small school system, a circumstance quite rare in anthropology and education (Firestone and Wacaster, 1976). In addition, the research project involved nine other studies of small communities that received NIE funds for educational change. When the final reports are completed, it will be possible to synthesize and compare the experiences of these ten school systems and communities as they participated in this huge educational experiment.

Research Design in the Ethnographic Process

A good research design would show the two basic phases of doing an ethnographic study of education: the exploratory phase and the intensive examination phase. One of the power sources an ethnographic study brings to education is a commitment to doing research which explores a school system and community unencumbered by *a priori* assumptions about what is most important to look at. It would be naive to suggest that a researcher could enter into a study with no prior assumptions, interests, or predelictions, but it is possible to hold these in abeyance during the period of exploration.

Designing an exploration period in a study serves two functions. It first of all allows the researcher to devise problems which take into account the unique circumstances of the study site. It allows for subsequent study based on inductively gained knowledge of a school, a district, or a community. A time of exploratory research provides investigators a chance to "de-center" (Werner and Campbell, 1973), a process by which the symbols and relationships of their own culture are replaced by those of the culture under consideration. De-centering in a school system within the United States is a more delicate and subtle process than the usual trauma an ethnographer faces in meeting people who share neither language nor experience with academic types (Wolcott, 1971).

The second function the exploratory phase serves is that it allows the researcher to gather as much contextual and comparative data as possible before personal, professional, or institutional limits are set on what will be the focus of a study. The exploratory phase allows the researcher to seek out and bring to light the background of cultural and social forces which have made the institution or community what it is. In some

cases, the information gained in such an exploration will do little besides set the stage for other parts of the study. More often than not, however, the historical, environmental, or other contextual knowledge gained in this way will prove to be central to understanding whatever educational segment is under study.

In the Abt Associates project I have been referring to, the period of exploration was designed into the research. It was expected to last approximately six months and the major product would be a "Site History and Context Study" (Burns, 1975). In Willcox, the historical and contextual review of the community and schools gave us the idea of a "cargo cult,"[5] a system of people and facilities which has the function of luring urban riches to this rural community. The fact that the school system received a million dollars for five years (about 15% of the total school budget for that time) to attempt comprehensive change is a good example of how the "cargo cult" worked. Like the cargo cults of New Guinea, the figurative runways leading to Willcox were not as equipped to handle the cargo as they were at enticing the planes to land.

The second phase of a study making up a research design is the phase of intensive examination of major topics. Levine (1973) has labeled this the "problem-solving or hypothesis-testing phase." The problem I see with his term is that presumably some problems may be solved in the exploratory phase, just as some hypotheses might be tested. During this second phase a study is brought into focus. In Willcox, the second phase concentrated on the way the government funds were transformed into a series of local project components such as bilingual education, early childhood education, community/schools, and local evaluation. The study of project introduction and implementation (as opposed to project impact) required careful observation of how the various components were staffed, how the new staff and positions were integrated into the formal and informal networks of teachers and administrators, and how the different "coordinators" of the components carved out places for themselves in the community. The funding agency, the National Institute of Education's Experimental Schools division, became a major research phenomenon, a topic to be investigated along with the activities of local people.

The formal research design for the study existed as a portion of the overall plan that Abt Associates had for studying the Experimental Schools projects in all ten rural school systems. The ethnographic studies were referred to as "case studies," and were described as broadly-based ethnographies which would tell the prospective reader "what it was like" to live through the local projects. Models for the studies ranged from Levi-Strauss' *Tristes Tropiques* to Hollingshead's *Elmtown's Youth*, the variety being built in to accommodate the range of interests among the eleven fieldworkers hired to do the studies (Fitzsimmons, 1975).

It is difficult to strike a balance in a research design which indicates both the deductive and inductive knowledge a researcher expects to use throughout a study. Ideally, a research design should be a concise plan which allows one to budget time and resources in such a way as to achieve a good study. If the problems are too carefully delineated at the outset, there is a danger that in-the-field observations may prove the problem to be non-existent. On the other hand, if aspects of schooling or education are not discussed in research design, there is the danger that the investigator may never get around to studying them or that funds for studying them may be channeled elsewhere.

There were no requirements on the Abt project for creating individual research designs beyond the general plan for the whole project. Each fieldworker was expected to design a study according to the standards of the discipline involved (anthropology, sociology, and educational administration), but no formal document was required. In my case, I wrote up a design for the study of Willcox six months after I had begun fieldwork. The design was an outline for a final report—a grand ethnography (in the traditional sense) that would cover topics ranging from prehistory to cultural change. In retrospect, the design memorandum I wrote was far too ambitious to carry out, but it did serve to bring some order to the field notes, photographs, interview transcriptions, and community artifacts accumulating in the office.

Anthropologists of education are not known for writing comprehensive research designs. In the general field of anthropology, more and more attention is being paid to this activity (Spain and Brim, 1974; Spindler and Goldschmidt, 1973), so perhaps the experience I have related here is anachronistic. Broadly viewed, the important aspects of research design were still present: an allocation of time and resources for both exploratory and intensive topic study, a statement of purpose about what was to be studied and how knowledge was to be gained, an indication of the product that would result from the study, and the relationship of the study to other literature in the field.

Development of Research in the Ethnographic Process

Earlier, I mentioned the importance of paying attention to the changes that take place in the conditions of research. Doing an anthropological study of education in an institution as complex as a school demands an incredible amount of "fancy footwork," or changes in the role of the investigator, for a study to be carried out. Not only do the agendas of a school system change from year to year with the vagaries of new administrations, new fads in education, and new federal programs to compete for, but the wider conditions of research change as well. The study I did of the Willcox Experimental Schools project is a case in point. Between

the first and third year of research, over twenty changes took place in the staff positions of the local project; at the Abt project level, fieldwork was shortened from five to three years and the ethnographic case studies were relegated to part-time status. The funding agency, NIE, underwent several near-crises and the Experimental Schools program lost much of its early interest within the agency. As a result of these and similar changes, the study planned for Willcox became more limited in scope. The goal of a complete ethnographic description was reduced to an account of how the local project operated.

Doing the fieldwork for the study was time-consuming, exhausting, and subject to the usual problems that someone doing participant recording, documentation, and observation of a school and community might face. The only point that needs to be made about fieldwork is that changes in the fieldwork's role should be expected. Friendships with key actors in the drama of education in a school become the basis for in-depth "expert informant" or field assistant relationships with people. While a fieldworker may begin a study attempting to be a detached and neutral "objective observer," such a pose is often not functional when information is needed on such topics as the basis for inter-organizational conflict or the secret lives of teenagers. The trick, again, becomes one of balance and the careful examination of the consequences each evolutionary change has on the role of the fieldworker. It is here that prior experience of fieldwork in an exotic setting is of value. Such an experience usually has the effect of humbling anthropologists enough to make it unlikely that they will become known as "troublemakers" in the culture of schools through outlandish behavior.

Managing the ever-growing storehouse of data from a study deserves serious consideration. If a study involves the storing and manipulation of a large number of easily codable data, then it is possible to take a portable key-punch machine to the field and transform notes directly onto computer cards. A far more common practice involves keeping two sets of notes, one a chronological log of events, activities, and interviews held with people, and the second a filing of relevant portions of the chronological log under the headings of the proposed final study. As I began the study of Willcox, I used this latter method. I had different file drawers for information on the schools and the community, each subdivided into categories that made sense in Willcox. A separate file drawer was reserved for the short papers, memoranda, and articles which would be integrated into the final study. Each file drawer also contained notes from articles and books that pertained to the topics I was investigating. In addition, a master card file was started which listed the materials for quick reference.

By the second year of research, it became apparent that neither this system nor the form of the final product as I had envisioned it would suffice. The card system was fine for noting the location of the synchronic data that filled the file cabinets but could not be easily adapted to referencing the chronological field journals that had been written. The idea of a "grand ethnography" also seemed out of step with the realities of the study I was doing. The local project and community were extremely complex. Trying to integrate some of the diverse aspects of the study between two covers seemed counter-productive to a goal I held of expressing some of this diversity in the report. I decided that the "case study" of Willcox could best be written if it contained a number of reports. One would be a major report of the implementation of the local Experimental Schools project; others would deal with specific components such as bilingual education or with methodological aspects of the study.

As the study changed in form and different sections of the reports began to be written, I found that edge-punched cards provided a means from moving from the files and journals of field notes and community artifacts to written products. These edge-punched cards served the function of distilling, organizing, and recalling information from the diverse sources of field data so that different reports could be written. The edge-punched cards had the disadvantage of being too small for some data. In addition, data such as photographic files could not be entered into the edge-punch system.

Product Creation in the Ethnographic Process

Making the products of a research effort available to the public is too important a job to be left to the end of a project. Although there is a long tradition of waiting until fieldwork is complete before the writing of a report begins, such a strategy can prove to be dysfunctional. In many circumstances, writing up a study must take place while other means of making a living are pursued, such as teaching or other research. By beginning the serious task of writing up a study as soon as one enters the field, it is possible to build up a kind of library of interim reports, papers, and chapters from the study which can be referred to, revised, and checked against future findings of the study.

Such an approach is followed informally by most ethnographers. The tentative models, letters to friends and colleagues, and research memos make up a body of literature which essentially serve this function of product creation in many studies. What I am calling for here is a more formalized procedure in which such products would be written up with the express purpose of disseminating them among local residents, colleagues, and perhaps even the policy audiences of a study. Such a strategy would seem especially useful on a long-term project where much anxiety arises from the lack of visible results of an ethnographic study. In addition, writing up interim reports serves the investigator by

providing a chance to pull back from the demands of day-to-day data collection and review how the total study is progressing. It also serves the investigator by insuring that an audience or constituency is developed for the kind of study being carried out.

The difficulty with producing study findings in mid-stream is that the confidence of the community under study might be jeopardized. In the study of Willcox, assurances were made when the study began that the summative research findings would not be used by the Experimental Schools staff of the NIE to make formative decisions about the local project. There are several strategies one can take in such a sensitive policy context. For example, the interim findings can be written in such a way that they deal with non-programmatic concerns such as the social make-up of a community, a "day in the life" of a student showing the place and attitude toward school in such a day, or the network of communication in a school. A second strategy is to write up the methodological problems and prospects of early fieldwork in an effort to examine the kind of objectivity brought to the field and also to provide the discipline with some examples of new field techniques. A third strategy is to write about the historical antecedents of the school or school system and the place of historical trends in the present-day world. In all cases, from early essays on fieldwork to final substantive reports, local school people, residents, and other consultants should review and critique the products. Not only does this insure that glaring oversights will be corrected but it also maintains good relations with the community under study. In the future, the discipline of anthropology and education will turn more and more to the re-study of schools and communities in an effort to collect a comparative set of studies which could test hypotheses about the differences between investigators or changes in education over time in one setting. Such important work can only be done if anthropologists and other researchers of education are careful to maintain good relations with the subjects of their studies.

Written reports are only one kind of product that can accrue from a study of education. To be sure, the tradition of scholarship gives a high priority to written results, and their creation is facilitated by the chances an investigator has of reading preliminary drafts at conferences and meetings and sending around photocopies to colleagues for review and criticism. But other forms for research products are available as well. Rob Walker and Clem Adelman have just published *A Guide to Classroom Observation* (1975) which shows the powerful use photography can be put to in educational research and teacher training programs. Video and audio recordings have long been used as technological aids to gathering data. If the expertise in graphic design and sound is available, these media could well be used for making public the results of studies.

Even if the traditional medium of prose is adhered to, there are many possibilities for publicizing results. *Scientific American* and *Psychology Today* are two popular journals capable of reaching a wider and different audience than are the more esoteric research journals of the trade. If an attempt is being made to influence policy, then it would prove beneficial to publish summaries and articles on research in those journals that policy makers are likely to read.

Rethinking the Ethnographic Process

Much of the tone of this discussion of the ethnographic process has been prescriptive. The ideas and suggestions I have presented here have grown out of my own experience on a large-scale, multi-disciplinary project carried out by a private research company. The experience has indicated how little the anthropology of education is understood outside the boundaries of the field. Part of this problem has arisen from the lack of attention many researchers have given to explaining the nature of what I have termed here as the "ethnographic process."

It is much easier to look back on a project and discuss what should have been done rather than look forward and plan for the future. Still, the exercise is useful in preventing future blunders. It is for this reason I used illustrations of my study to talk about the ethnographic process in general. The discussion of the conditions, design, development, and product creation of an ethnographic study of education presented here is meant to begin a dialogue which will strengthen the way research is carried out and publicized. This essay is meant to be a first word—not the last—in what I hope will be a lively conversation.

Notes

1. I would like to acknowledge the support of Abt Associates and the National Institute of Education (contract OEC-72-5245) for the opportunity to do the research which forms the basis of this essay. Without the support of colleagues and friends on the project staff, writing this would not have been possible. Terence Hays, Harry Wolcott, and Homer Barnett made many useful suggestions on earlier drafts, but the responsibility for the interpretations and shortcomings of this paper remain with me and not with these august consultants or with Abt Associates.

2. The allusion to the popular TV series *Star Trek* is based on a high school teacher's remark about my study when he heard me explain it soon after I arrived in Willcox. He compared Abt Associates with the crew of the spaceship whose mission it was to "find new life in these distant school districts but not interfere with the aliens."

3. This possibility looms as very probably because the reliability of paper and pencil questionnaires is under

study and response rates decline.

4. The Experimental Schools program of NIE made a distinction between local, formative evaluation of the projects and the summative evaluation carried out by the Abt Associates research team for all ten rural school systems funded by the agency.

5. Harry Wolcott first recognized this similarity between Willcox and New Guinea; his insight is highly appreciated.

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PRESIDENT'S REPORT

Three topics will be discussed in the present report: a Dire Need, an Imminent Achievement, and a Long-Sought Union.

The Dire Need is for an editor of our *Quarterly* to replace the distinguished incumbent, Jack Chilcott, who seeks the flexibility for other endeavors (see below) that becoming a former editor will bring him. Jack will thus leave his post after the November 1976 issue, completing a three-year term of office.

So—the search is on for the third editor of the *Quarterly* (John Singleton was the first). The task is demanding but has its attractions. Perhaps foremost is the opportunity for central participation in the continuing definition of the subfield of anthropology and education: by what and how the editor solicits, commissions, and chooses to publish, he or she is telling the world what educational anthropology is or ought to be. The editor's influence in this regard is more intense (four times a year!) and sustained than that of CAE's officers, who may shuttle through their posts several times during the editor's maximum three years on duty. With the editorship comes correspondence, too, with producing anthropologists and educationists throughout the United States and the world and access to up-to-date information about current projects and publications in this subfield.

The editor is a non-voting member of the CAE Board of Directors, and chairs a personally selected editorial board. There is hard work involved, too, such as cajoling tardy contributors, proofreading, arranging for typesetting the manuscripts, designing layouts, negotiating with the printer, and so forth. Would-be tyros should probably seek from their university or employer assurance of released time (one-quarter to one-third) and assignment of a professional typesetter. In these financially difficult times, perhaps this is an unrealistic expectation and alternative means of dealing with the demands on the editor's time and skills should be considered. If you are interested in assuming the editorship, we invite you to tell us how you think you will be able to handle the "production" phase of the position.

The "we" in the preceding sentence refers to the Search Committee members I have asked to assist in the selection of the new editor: Paul Carlson, University of Houston, Victoria Center; Bud Khleif, University of New Hampshire; Frances Schwartz, Swarthmore College; and Richard Warren, University of Kentucky.

Adademic protocol formerly required aspirants for a coveted position to languish demurely until someone else had the extrasensory perception or blind luck to nominate them for the desired sinecure. Well, CAE certainly didn't grow that way, and if guiding the *Quarterly* through its next stage or stages of development appeals to you as a means of advancing the subfield, expressing your creativity, and making a mark for yourself within the profession, *for Pete's sake, let one of us know right away!* And if you have a friend who seems well-qualified but just a bit bashful, send us your friend's name, too. We'll handle the rest!

I mentioned at the beginning certain "other endeavors" that Jack Chilcott hopes to attend to, his term of office completed. In part, these consist of research and writing of his own, and in part of making appropriate use of opportunities for the good life in his Tucson home territory. But Jack also hopes to undertake active exploration of a CAE monograph series, in which he has a long-standing interest. Many members of the Board of Directors hope that Jack can devise financially and academically sound plans for such an enterprise. It will be a challenging assignment, however—witness the ups and downs of AAA's monograph series—for which the continuous duties of getting out the *Quarterly* provide insufficient leisure. In leaving the editorship, Jack is really being unleashed, a fact of which he seems thoroughly aware. Thanks, Jack!

The Imminent Achievement, probably recent history by the time you read this report but still pending as I write it, is essentially Bob Textor's, on behalf of CAE. I am referring to the invitational "Monterey Conference" (July 21-23) on the uses of ethnography in research on education. (The formal title of the meeting is "Workshop Exploring Qualitative/Quantitative Research Methodology in Education," a set of phrases worthy of instant oblivion.) As reported in the February *Quarterly*, the Far West Laboratory approached CAE last December to co-sponsor such a meeting, to be funded in its entirety by NIE, and the Board of Directors approved the venture. Textor was appointed our representative and negotiated (entirely pleasantly, he reports) through the winter and spring. CAE ideas and perspective are firmly entrenched in the final program.

CAE and educational anthropology will be represented by Ray Rist, NIE; Eleanor Leacock, CUNY-Brooklyn; Louis Smith, Washington; Fred Erickson, Harvard; Robert Herriott, Abt Associates; Courtney Cazden, Harvard; Dell Hymes, Pennsylvania; Textor, and me. Another main contingent will be composed of "metricians," to employ the Lab's useful neologism: psychology, tests, and numbers researchers, more representative than we anthropologists are of the education research community. A third group will consist of foundation officers and government officials whom we hope to "educate" about the potential of ethnographic techniques and findings for comprehending and solving educational problems.

The strategy for this conference is to eschew the

broad view and to concentrate, via prepared papers (one by an anthropologist, one by a metrician), on specific substantive topics for which interdisciplinary collaboration may be illuminating. Topics chosen include "Assessing Race Relations in the Classroom," "How to Identify Effective Teaching," "Next Steps in Qualitative Data Collection," "Why Do Demonstrations: What Can Be Learned," and "Assessing Language Development: Written/Oral." Our ambitions are high—we expect that the Monterey Conference of 1976 will be as influential to the growth of our field as the Stanford Conference (organized by George Spindler in 1954) and the Miami Conference (organized by Fred Gearing in 1968) were.

One direct benefit for CAE members will be receipt of a free copy of the conference proceedings as soon as they are available, a perquisite of membership in the society. These will be published by CAE under the terms of our agreement with the Far West Lab and NIE, and distributed widely to libraries and other professional organizations in addition to CAE members. By this means, CAE's activities and interests will be made known to a wide range of potential members and collaborators.

Finally, the Long-Sought Union will take place (if long-laid plans materialize) in Washington, November 17-21, at the AAA-CAE Annual Meetings, when, for the first time, high school and elementary school teachers of anthropology will participate in the Meetings in representative numbers. Do you realize that in 1974 there were 2901 teachers of anthropology in American high schools (not all of them full-time in anthropology, of course), 103 of them located in the DC-Maryland-Virginia area?

The major thrust of my presidential year has been and will continue to be the "bringing in" to CAE of those high school and elementary teachers now presenting the subject in the schools who, for one reason or another, are not affiliated with CAE or AAA. Some may feel that CAE is the plaything of the university research community; others may feel that CAE cannot speak to their specialized, instruction-oriented interests; a number may anticipate a patronizing attitude towards them by current CAE members. It is most unlikely that the majority has not heard of CAE and its openness to contributions of new interest groups. Certainly the participation of large numbers of high school and elementary school teachers in the affairs of CAE would signify the advent of a new interest group! The Board of Directors and I hope to end this separation.

A significant beginning to the end was made last year in San Francisco, with the invaluable assistance of the Bay Area Teachers of Anthropology and Bay Area Archeological Collaborative. Yet the number of teachers who came to the Meetings and participated in CAE events was disappointing. In Washington, we will have additional bait. Walter Watson, co-chairperson of Committee 3 (Anthropological Resources and Teaching) has arranged a display of elementary, secondary, and junior college curriculum materials (textbooks, true, but especially items other than textbooks) that will be located in special space added to the regular book exhibits for the full duration of the Meetings. Quite intentionally, many of the CAE symposia included in the AAA Program deal with the teaching of anthropology or subjects of potential direct concern to teachers in sub-collegiate institutions, as do many of the regular AAA sessions.

To inform our school colleagues of these and other opportunities at the Meetings, a regular blizzard of publicity will go out in September and October. Some of this will be dittoed announcements; other parts will be transmitted informally via the network of contacts in the Washington area that we have assembled in the months since December last. Our goal will be to convince these teachers that they are wanted at both the formal and informal sessions and that they will find them helpful in improving their teaching effectiveness and anthropological understanding. We hope that similar efforts to reach local teachers will occur at subsequent CAE-AAA Meetings (e.g., Houston in 1977) so that, before long, CAE is in touch with practicing schoolpeople in all parts of the country.

A "Union" with our scarcely known brethren offers many potential advantages for all parties—knowledge of instructional techniques developed by high school teachers, opportunities for institutional and community research, enrichment of the substantive background of pre-collegiate teachers, further development of public understanding of anthropology, consulting jobs, participation in the expanding arena of in-service teacher education, new members for CAE, communication among otherwise isolated instructors of our discipline, and many others. I hope that in the bag of tricks planned for Washington we have included ones that will be effective in bringing us face-to-face with our separated colleagues.

John D. Herzog

THE CAE *QUARTERLY* 1973-76: THE LIFE CYCLE OF AN EDITOR

John H. Chilcott

Following the model of role theory, I must admit that I was most uncertain as to the expectations for a CAE editor three years ago when I assumed the editorship of the *Newsletter* (as it was then called). The role of editor had not been (and still may not be) well defined. As is true of any individual moving into a new role, I attempted to determine what the CAE membership expected from their publication. Before I join that contingency of happily smiling retired CAE officers, I thought I would like to share (with those of the CAE readership who are interested) some of my frustration, sense of accomplishment, and impressions of the future of CAE.

One of my first actions as editor was to summarize the previous accomplishments of the CAE *Newsletter* (November 1973, p. 10). At that time I perceived four major areas of CAE interest: (1) An interest in the cultural context of the school of American society; (2) An interest in the education of ethnic minorities, particularly in the U.S. and Canada; (3) An interest in the role of the school in both macro- and micro-cultural change in foreign countries; and (4) An interest in the use of anthropological data and theory in the training of teachers. All four of these interests have been continued through the publication of a wide variety on these topics during the past three years.

One of my major concerns when I became editor was the apparent isolation between the anthropologist in an academic setting and the professional educator in the school. Early issues of the *Newsletter* consisted primarily of conversations between anthropologists rather than dialogues between educators and anthropologists. I'm sorry to report that not much progress has been made in this direction. To be sure, some recent issues of the *Quarterly* have described the role of anthropologists in non-academic settings, most of whom express a very high level of frustration; but there has yet to be much concern for educators struggling with putting into practice their anthropological training. The recent survey of the CAE membership reveals that a large number of CAE members are not anthropologists, yet somehow these individuals have not been adequately represented in the *Quarterly*.

A colleague of mine, the late Edward P. Dozier, once remarked in a faculty meeting that all of anthropology is applied anthropology. I would alter this statement somewhat to read that all of anthropology *can* be applied anthropology. Anthropology needs to be translated into action for educators, not an easy task because it requires a fundamental knowledge of anthropology and a creative mind to make this transplantation. Much of the anthropology I have seen educators attempt to translate has been most inadequate. This, I might suggest, is not so much the fault of educators as that of the anthropologists who have not provided much in the way of a model for making this transposal.

This may, indeed, be a dead issue since none of the recent candidates for CAE office have expressed this concern. Privately, however, I have received a number of letters from individuals who are concerned about the relationship between educators and CAE, and particularly our parent organization, the AAA. This division became eminently obvious at our meetings in San Francisco where a number of interested teachers were excluded from the sessions.

John Herzog, in his President's report for this issue, describes some movement at the forthcoming AAA meetings in Washington to alleviate this situation. Our attention will be directed to the AAA registration desk to watch John's success in this endeavor.

The relationship of educators to CAE may be more of a political issue than a publication issue—yet publication policy should follow political policy. Consequently, I would like to make some suggestions to the Steering Committee.

First, I would suggest that the location of the annual meeting might be re-examined in terms of the continued conflicts, both as individuals and as a group, at the AAA meetings. I'm not suggesting a split from the AAA but I would suggest that only the business affairs of CAE be conducted at the annual meetings and that CAE papers, symposia, and the like, be presented at the annual meeting of the Society for Applied Anthropology (SfAA). This scheme would permit anthropologists to pursue their academic interests at the AAA, and allow both educators and anthropologists to pursue their *mutual* interests at the SfAA. It has been my impression that the SfAA has been more amenable to "outside" participation in their meetings.

For the anthropologists, this scheme would have the advantage of permitting them to participate with their colleagues at the annual AAA meetings in generating new knowledge in the field of anthropology, some of which, no doubt, would be related to education. For the educators, this scheme would permit them to participate along with anthropologists in the translation of this new knowledge into educational practice. If the Standing Committees conducted their meetings at the SfAA, this would permit them more time and allow them to include among their members more individuals who have an

interest in their particular activities.

Finally, it occurs to me that CAE members need to move out of the friendly portico of their own organization into the "native" world of teachers, administrators, and informal educational settings. We need to participate in workshops, organizations, and consultantships where CAE is the minority. President John Herzog in this issue describes some of CAE attempts to perform this function. To date we have, more often than not, expected educators to come to us rather than our going to them.

We need to help organize and participate in the activities of local educational organizations—not only as resource people in curriculum development but as specialists in problem-solving at all levels of education. Care should be exercised here, as there are some problems anthropologists cannot solve.

We are particularly fortunate here in Tucson to have a local organization of anthropologists (Society of Professional Anthropologists) who all work in non-academic settings. I've found participation in this organization particularly interesting, for even though the members may be working in public health, model cities, alcoholic rehabilitation, or local television, they are all involved in cross-cultural communication and education.

Accounts of CAE participation in these organizations would provide models for other CAE members and other educator organizations in translating anthropology into educational practice. Such activities would be most appropriate material for the *Quarterly*.

Some years ago, Fred Gearing (May 1971, p. 17) suggested that the world of anthropologists and the world of educators are different, particularly with respect to their different reward systems, goals, and ideologies; and yet there were points of overlapping common interests between the two groups. I would suggest that the four major areas of CAE interest previously acknowledged represent these common interests.

During the past three years, I have attempted to incorporate all these interests within the framework of one publication. The appointment of an editorial board has been most useful in helping with this purpose. Additionally, the appointment of two special editors—one concerned with teaching anthropology at the college level and another concerned with teaching anthropology and education courses—has proved to be most useful in broadening the interests of the readership. However, there is still considerable concern that the *Quarterly* is trying to be all things to all members. There is still a group of CAE members who would like to see the *Quarterly* remain a publication solely for research and theory in anthropology and education, while yet another group would prefer to emphasize the applied aspects of anthropology and education and leave the reporting of research to other anthropological journals.

The Editorial Board has attempted a compromise between these two positions by developing a policy which maintains the *Quarterly* much as you see it—a service-type journal with articles and news items of interest to the diverse CAE membership. We also are currently developing a monograph series which will contain articles of significant research and thinking in the field of anthropology and education.

To this end, CAE is fortunate in participating in a workshop sponsored by the Far West Educational Laboratory, the papers from which will be published as the first monograph in this series. As funds become available, other monographs should follow. The August and the November issues of the *Quarterly* contain papers which were originally designed for a monograph series that remained dormant due to lack of funds.

I have been most happy with the special editions of the *Quarterly* sponsored by the standing committees. Next February, the newly reorganized committee No. 3, "Anthropological Resources and Training," will edit an issue of the *Quarterly*, and next May, committee No. 7, "Blacks in Education," will edit their issue. I hope that this practice will continue.

Some topics I would have liked to address in the *Quarterly* but didn't, would include the following:

An examination of some of the major educational issues confronting the public in the media. Can anthropology solve the busing issue? If not, then what contribution could an anthropologist make? How would an anthropologist explain the rise of conservative education—back to basics—in an era of rapid socio-cultural change? Can, indeed, anthropologists train teachers to become "culturally sensitive"? If an anthropologist could create an IQ test, what would it be like? Finally, why has the anthropologist, supposedly trained in human biology, become so reluctant to relate human genetics to human learning? In short, what I think is needed in future issues of the *Quarterly* is solutions to educational problems rather than descriptions of these problems.

According to Henry Burger, sociologist Phillip Foster recently stated that no significant advances in theory have been generated in anthropology and education during the past five years. Is this true? Several years ago, the retiring Dean of the College of Education at Harvard stated that education had no theory. Is this also true? I might suggest that both statements are true in the sense that any theory in anthropology and education or in education will be generic to a specific discipline, rather than to education itself, since education is a cultural process rather than a discipline. Thus, any theory in anthropology and education must come from anthropology. Up to now, I have not seen specific anthropological theories applied to education. I would like to see an essay on how a "French structuralist" would view education. I would like to see an essay by a "cognitive

anthropologist" on how that person viewed education—or a "neo-evolutionist," or a "functionalist," or a "generative grammar" person. It is here that theory in anthropology and education might emerge, rather than in descriptive studies, most of which have little or no theoretical framework.

Not that anthropology and education is without any activity in this direction. During these past three years, I've attempted to provide reports on the research activities of such noteworthy persons as George Spindler (February 1974), Michael Cole (February 1974), Fred Gearing (May 1975), Frances Ianni (May 1974), Marion Dobbert (May 1975), Allan Howard (May 1976), and Thomas La Belle (November 1975). I hope the new editor will be more successful than I was in updating the work of Solon Kimball, Jacki Burnett, Rolland Paulston, Charles Harrington, Lombros Comitas, Estelle Fuchs, Fred Erickson, Murray Wax, and others.

At the same time, I attempted to provide a wide variety of articles—though primarily descriptive in content—in order to demonstrate the wide variety of interests in the field. The editorial board feels fortunate that a large number of scholars are now considering the *Quarterly* for their publications. The processing of these manuscripts is the one area of my editorship in which I feel a sense of failure. Too often, the reaction of the review committee has been too long in arriving. To those individuals whose manuscripts spent weeks—sometimes months—residing in my files (some are still there), I offer my apologies. Perhaps the new editor will be better organized than I was.

As I look back on what has been accomplished and what might become some concerns for future issues, a number of ideas emerge. First, I would like to see a reintroduction of cross-cultural studies of child-raising, an area which dominated the field of anthropology and education some 25 years ago when I first became interested in the field and which seems to have disappeared with the demise of Freudian psychology. In this regard, I would suggest some attention to child-raising practices among minorities and the cultural change processes and influences upon young parents to raise their children. It occurs to me that the results of this research will always remain invaluable to teachers, counselors, and parents.

Another area of interest might be the anthropological study of power in education, with particular emphasis upon the decision-making process. Some work has been initiated in this area but little has been published.

Additionally, I would suggest some work on the subject of educators as change agents. To be sure, there has been considerable description of education as a cultural change process, but little has been accomplished in developing guidelines for educators who wish to initiate change.

Finally, I am reminded that of the first textbooks in the field of anthropology and education two were written by philosophers—George Kneller and Theodore Brameld. What has happened to the relationship of anthropology to the philosophy of education? It is here that anthropologists' skills in ascertaining cultural goals would make a major contribution to philosophy, educational policy, and educational practice.

TEACHING ANTHROPOLOGY AT THE COLLEGE LEVEL

Dward A. Moore, Jr.
Special Editor

This section of the* Quarterly *is devoted to an exchange of ideas on the teaching of anthropology. Course descriptions, philosophical statements, reactions, and comments are welcome. Persons with material to contribute are requested to send them to the editor, Jamestown Community College, Jamestown NY 14701.

[Ed. Note: The two articles which appear below indicate the continuing efforts of anthropology faculty to offer new experiences to their students. Whitney and Dubbs describe yet another individualized approach to anthropology based mainly on Keller's "Personalized System of Instruction," their modification of this structure indicates its flexibility.

Lehavy presents us with ideas for offering physical anthropology as a laboratory course in which students receive laboratory science credit. This is an exciting idea for expanding the traditional offerings in anthropology, especially at the two-year college level.]

MODULAR FLEXIBILITY IN AN INDIVIDUALIZED INTRODUCTORY CULTURAL ANTHROPOLOGY COURSE

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Teaching anthropology, like doing anthropology, lends itself to a wide range of acceptable approaches. However, whatever approach is selected, instructors must give careful thought and consideration to at least two important areas of the educational process: course content and presentation techniques. It has been our experience that most new instructors have only recently completed long years of schooling in which virtually all course work was presented in either a lecture/discussion or seminar format, with the latter usually reserved for graduate level instruction. In part, these techniques result from the organization of instruction into time blocks of a specific duration, be they hours, quarters, or semester, and this system is then adhered to by most new instructors.

Recent articles in the CAE *Quarterly*, however, have indicated an increasing interest in altering the techniques of anthropological instruction along the lines of an individualized, personalized, or Keller approach (Moore, 1974; Sanford, 1975; Steffy, 1975). That one of the most individualized of disciplines is finally focusing on individualized instruction is, in our opinion, a belated but healthy sign. After employing numerous instructional approaches in our introductory courses over the past several years, we found ourselves tending toward greater student flexibility, greater individualization of course material, and an increasing reliance upon methods of giving students ways to "experience" anthropology while at the same time reading about it.

About two years ago we came across the Keller instructional approach and discovered that much of what we had developed by trial and error was very similar to the Keller approach or, as it has come to be known, Personalized System of Instruction (PSI). We then set out to see if and how the PSI approach could be adapted to both introductory cultural anthropology material and, of equal importance, to the lock-step 60- or 75-minute time period, one instructor to one class, administrative arrangement of classes at San Diego State University. We have been able to modify the PSI approach to fit both of these considerations and, we are told by our students, with a good deal of success. The remainder of this article describes some of our early attempts, some of the pedagogical problems we wrestled with, and our latest modification of the PSI approach.

An Early Attempt

In our first individualized class, we relied heavily upon Green's (1974) compilation of PSI materials in order to design our course. We then obtained funds from the San Diego State University Teaching and Learning Council to send one of our graduate students, Mr. Richard Anderson, to a PSI workshop. When he returned with PSI information, we set about ascertaining the main features of PSI and how they could be adapted to our local situation. The main features are:

(1) The removal of the lecturing teacher as a major source of course content and the organization of the course material into self-instruction units, with a related stress upon the written word in teacher-student communication.

(2) The go-at-your-own-pace feature, which permits students to move through the course at a speed commensurate with their ability and other demands upon their time.

(3) The mastery concepts, or unit-perfection requirement for advancing, which lets students go ahead to new material only after demonstrating mastery of that which preceded.

(4) The use of proctors, which permits repeated testing, immediate scoring, almost unavoidable tutoring, and a marked enchancement of the personal-social aspect of the educational process.

(5) The use of lectures and demonstrations as vehicles of motivation rather than as sources of critical information (Steffy, 1975).

In principle, we agreed with all five features: however, we determined we could realistically implement only 1, 2, 3, and 5. We decided not to employ proctors in our initial attempt at PSI primarily because we wanted to gain first-hand experience with the technique ourselves, and we could not envisage under our administrative system a suitable academic reward for individuals who might volunteer their services as proctors. Thus, we, with Anderson's assistance, did all the proctoring and tutoring.

For the basic core of our course, we constructed a progressive series of twelve required modules (units) that required mastery through testing. Those modules required mastery in the following sequence in order to obtain, depending upon the level of mastery, either a B or C grade in the course: (1) Nature of Anthropology; (2) Fieldwork; (3) Culture, Social Structure, and Environment; (4) Language; (5) Enculturation; (6) Social Structure; (7) Midterm Review; (8) Economics; (9) Socio-Political Control; (10) Religion; (11) Culture and Personality; (12) Change.

With the exception of Unit 11, which required us to write a brief essay as source material, we used standard texts (Taylor, 1973; Bowen, 1964; Powdermaker, 1966) coupled with films or videotapes as source material for each unit objective. To encourage variable grade possibilities and course experiences, we designed and added seven optional units, with certain required units as prerequisites, involving written reports about non-class-

room activities. These "field activities" were projects involving the collection and analysis of data obtained in the San Diego area and resembled the type of projects described in Crane and Angrosino (1974), Maranda (1972), and Schwartz, Sowek and Cowan (1971). Believing that "enrichment" lectures would allow for a profitable group discussion on topics given scanty coverage in our unit source material, we also scheduled four such lectures. Both an early and a regularly scheduled final examination were optional.

Although most of our students were able to jump the required twelve hurdles in the span of a 15-week semester and believed the course to be a welcome change from the usual lecture/discussion course, we found through our discussions with them and their written, anonymous evaluations that the most frequent complaint was a sense of mechanistic programming, i.e., two must be done before three, six before seven, and so forth. Having been involved in lecture/participation courses both as students and professors, we shared some of our students' concern about the regimentation involved in our modified PSI course. Rather than completely abandon our attempt at individualization, however, we decided to re-examine our goals for introductory cultural anthropology and the PSI method as used by us to see if we could eliminate the mechanistic features of the course.

Lecturing vs. Individualization

One of the most difficult steps in individualizing is to accept the ego-threat involved in the abandonment of giving lectures. We all like to think we are excellent instructors and stimulating lecturers. Even though accepting these gratuitous self-perceptions, we still need to ask, "What do we teach in introductory cultural anthropology?"; and of even greater complexity, "Why?" More often than not, we believe most introductory courses tend to follow a standard format of topics and the subject matter conveyed, except for "war stories" and personal elaborations, is adequately available in any one of several dozen texts.

Why, then, do we as instructors usually lecture to large groups of diversified students about material that generally is available in print? We strongly suspect the answer is that we listened to lectures as students and therefore perceive this to be the proper medium for university instruction. Most university instructors are indeed capable of delivering a number of lectures that are both stimulating and informative and which may not cover ground also covered in texts. But how many can sustain this high quality of lecturing over a period of 10 or 15 weeks? Isn't it common to feel that perhaps one-third of the students is bored to tears by a lecture because they already know the material, that another third is bored because it is over their head, and, if we are lucky, the final third is really stimulated and interested in what we are saying? Is it possible to lecture to 50 or more students and reach them all, recognizing they come from various disciplines and have a wide range of reading and intellectual backgrounds? We think the answers are self-evident, at least from our experiences at San Diego State University.

While an individualized approach minimizes the personal gratification or ego-enhancement associated with a lecture course, we believe that individualizing allows excellent instructors to become even better, and stimulating lecturers to be more interesting in student-teacher interaction. Why? An individualized course does not necessarily change the instructors but it does change the all-important relationship between the instructors and their students. Individualization allows instructors to respond directly to the needs of each individual student. There is still ample opportunity to relate "war stories" and personal elaborations but it is done in a context that is meaningful to that particular student, the one who will benefit from it. All students are individuals, with individual problems and understanding, and instructors can take advantage of this in the numerous individual conferences throughout the semester.

Pedagogical Concerns

Analysis of our first attempt at individualization convinced us that the Keller plan's "removal of the lecturing teacher as a major source of course content" was not only viable but better, so long as it included carefully drawn and specifically written self-instruction course materials emphasizing small, readily mastered units. But what of course content? How could we eliminate mechanistic tedium and occasional procrastination so evident in our first attempt? Similar to Dobbert (1972), we were committed to the notion that the major goal of our course should, insofar as possible, reflect the needs and interests of our students. At our institution, introductory cultural anthropology fulfills the general education requirements, so the vast majority of our students are not anthropology majors, never intend to become majors, nor engage in the study of another culture. Thus, in introductory cultural anthropology, we hoped to instill in our students a general anthropological perspective similar to what Albert (1963) called the "anthropological point of view." Such a perspective would be of value to them in understanding and appreciating other peoples and cultures and thereby lead to a better understanding of their own culture and themselves. As Albert so aptly wrote:

> *It is, then, not really venturing very far to suggest that there are distinctive core conceptions of man and of methods of studying man that constitute 'the' anthropological point of view. . . . [It] may best be regarded as a developing conceptual framework within which communication can oc-*

cur among anthropologists of diverse interests, theories and methods–and, hopefully, among students of anthropology in India, Norway or the United States.

While a formidable goal, it seemed worthy of pursuit, though instructors teaching anthropology majors might well find fault with it since the "standard" ethnographic facts and other minutiae are minimized and general principles and concepts are emphasized. In our pre-individualized courses, we encouraged conceptual understanding by having students collect and analyze ethnographic data rather than rely on any one of the numerous ethnographies available for instructional purposes. If students wanted or needed other ethnographic data, we believed them capable of pursuing it on their own for whatever purpose.

We adhere to the belief that the introductory course in cultural anthropology should expose students to the subject matter, methods, and explanatory systems of our discipline so that students might develop the "anthropological point of view." However, we do not attempt to mass produce a covey of "miniature professionals" (Dobbert, 1972). While Dobbert logically argues that few students actually analyze *a* society once they finish an introductory course, we feel the development of an anthropological perspective allows students to analyze their own society and, more importantly, their relationship to that society. Suffice it to say that memorization and eventual regurgitation of a bundle of ethnographic facts is not our goal. We envision the discipline as an active field of study, with involvement as a prerequisite for learning and understanding. While one could presumably learn anthropology from a book or lecture, we believe one cannot understand anthropology (i.e., develop the anthropological perspective) by these media alone. So, we searched for a method that would encourage active learning as understanding as opposed to passive learning as regurgitation. We came up with what we call the "modular flexibility approach."

Modular Flexibility, Mastery, and Self-Pacing

Modular flexibility is a system of instruction with two essential components: (1) course subject matter and materials are broken down into smaller units (modules) capable of mastery with a few days study or outside-the-classroom data collection and analysis; and (2) self-pacing, whereby students are allowed flexibility both as to the modules they undertake as well as the time they allocate to a module. The total course, then, consists of numerous individual modules which, although self-contained, are part of a carefully thought out whole–the introductory course itself. Students are permitted to set their own pace for completing modules and further to select the order in which to complete the modules, so long as mastery is evidenced prior to undertaking a new module.

Modular flexibility. Although we had incorporated the modular approach in our first PSI course, both we and our students felt the required serial progression through twelve units resulted in a rather mechanistic, unstimulating, learning model. The serial arrangement of various anthropological subjects was a "survival" from our earlier teaching when we presented subjects in some kind of seemingly logical order. As we analyzed this particular aspect of our course, we came to question the validity of such a serial, progressive organization. A cursory examination of anthropology textbooks and ethnographies revealed that authors indeed varied in their determination of what follows what within the broader scope of cultural anthropology. Virtually all possible permutations of arrangement seemed present in the various texts. For example, why does kinship come before economics? Language before political organization? Change before religion? We concluded that serialization was an arbitrary process that might be detrimental to our course goal in the sense that it could lead students to a segregated understanding of anthropology instead of an integrated understanding or perspective.

To a certain extent, we were also the victims of relying too heavily on the PSI method as explained in Green (1974). The traditional PSI approach seems to have been first developed and used for those subject areas which had a body of essential material that had to be mastered in step-like fashion: for example, one usually does not attempt to solve algebraic equations without first mastering the notion of equivalency. Does this apply to anthropology? We think not.

For two reasons, we felt it important to have a few required serial units. First, we believed that before attempting to understand material on political organization, religion, and the like, students should have been exposed to the concept of culture and its ramifications; textbooks were nearly unanimous on this point. We also believed the student should understand the anthropological method, i.e., fieldwork and data analysis; textbooks were somewhat less unanimous on this point. Second, since our course materials involved a variety of student activities–reading the book and being tested, conducting research and reporting, viewing and analyzing films, and so forth–we wanted students to sample some of the variety early in the course so they could decide whether they wished to continue in this course or try some other, more traditional anthropology class.

Students in our revised course were required to complete three units before going on. The required units were the concept of culture, the establishment of a contextual framework, and experience in collecting and analyzing cultural data. The first was mastered by reading the text and making 80% on an examination (combined objective and essay questions); the second by viewing a videotape on the Tasaday (Philippines) and answering a specific essay question; and the third by

reading the book, collecting data in and about San Diego, and presenting a 5- to 9-page typed report. The remaining topics could then be selected and completed in any order determined by the students.

That decision raised another question—need every student proceed through *all* the remaining sub-areas of anthropology? Is each sub-area necessary or relevant for a student not intending to major in anthropology? For example, does it serve a real purpose to require a religious studies major to master a unit related to economics? We decided that, while mastery of all the conceptual areas is indeed desirable, it needn't be required for us to attain our goal of imparting the "anthropological point of view." Therefore, we expanded our course units from 12 to 23, with more than one type of activity often tied to a particular sub-area, and allowed students to pick and choose those units they wanted to complete. Modules were of four types: testing of text materials; written reports based on field activities (often requiring text reading as well); written responses to audio-visual presentations (combined with material in the text); and self-designed modules over text materials. This four-fold feature was designed primarily to accommodate the different interests and performance abilities of each student.

Mastery. We had, then, adopted the Keller Plan's modular approach and extended it to allow for individualized student selection of materials without the lock-step seriality so characteristic of familiar courses. Keller's "mastery concept or unit-perfection requirement for advancing" was modified to eliminate the need to march in serial fashion. We did, however, retain the general mastery concept. All students were required to master a unit before undertaking another. Successful mastery was evidenced by attaining 80% or better on the work after a student-instructor conference. Students attaining less than 80% were then counseled on their weaknesses and required to repeat the work at a higher level.

Self-pacing and multiple exit. It was important, we felt, that the students be allowed to set their own pace for completing modules during the semester. Students have varying time to work on a specific course at different times in a semester; therefore, it is important that they be allowed some control over their allocation of time. We began with the assumption that each student was a mature adult, capable of deciding how much or how little time to allocate to various activities. One student may require a week or more to complete a module, whereas another may complete the same module in one evening. So we imposed no deadlines for module completion. We did, however, provide students with a recommended plan for maintaining a relatively steady pace throughout the semester, if they desired.

A further modification was to institute a multiple exit plan. Since students are not all equally motivated towards, or interested in, things anthropological, we assigned variable point awards for each unit depending on the level of difficulty and type of unit, and then set up an A-B-C grading scale based on the total accumulation of points. The "C" range represented what we believe to be the minimal anthropological understanding necessary to foster an anthropological point of view, while the "A" range represented a thorough command of the material. This approach allowed students to leave the course after successfully mastering sufficient units to accumulate the points for their desired grade. They could, in essence, decide early in the semester what grade they wanted and work towards that grade, being assured of it once they had enough points. Students failing to get enough points for a "C" grade were given "Fs." While contrary to the pure PSI approach, this did eliminate procrastination and also solved the problem created by the administrative requirement that courses be completed within one semester.

Lectures and Proctors

The Keller feature of using "lectures and demonstrations as vehicles of motivation" was dispensed with after one semester's trial. Attendance was extremely low. Then, too, because of the change to student selection of modules, it was impossible to design lectures based on student progress—they were everywhere. Furthermore, student comments indicated a general disinterest in lectures. We found we can more effectively impart the same information to students in the conferences when they feel it is of more interest to them.

As mentioned above, we also dispensed with the Keller feature of student proctors. We were able to handle all testing, evaluation, and counseling ourselves with the help of a graduate assistant. Our plan, though simple in practice, is difficult to describe. Both of us each scheduled two sections of about 50 students each (a grand total of about 200 students). The classes were scheduled consecutively in the same classroom. That gave us almost 6 hours in the same room, two days a week. Students were told they could come in any time during those hours to take tests or turn in projects, and have the work evaluated there. In this way, we were able to circumvent most of the restrictions imposed by the standard time block for classes. Students were seen on a first-come-first-served basis. So far, this method has worked without undue clogging or long waits. We spend anywhere from 5 minutes to 30 minutes with students, depending on the nature of their problem. Thus, while tutors may be a desired feature of the Keller plan, they certainly aren't indispensable.

Conclusion

In sum, our revisions produced an individualized course that has: (1) A series of individual study units

focusing on a variety of topics. These units explain the importance of the topic, the student goals for that topic, and a suggested procedure for attaining the specified goals. (2) An optional, individualized, self-paced schedule under which students decide what unit will be completed and when it will be completed, with the exception of the three required units at the beginning. (3) A cumulative point schedule which allows students to complete the course when they have achieved the grade they chose, though anything less than a C results in an F. (4) All class periods devoted to individual tests, project evaluation, counseling, or audio-visual presentations. Our implementation of modular flexibility and individualization has been a success in the view of students and in our own opinion.

Students felt they had participated in an individualized introductory course designed to accommodate their interest in anthropology without sacrificing learning content. Though some students dropped the course after realizing it would take more effort than lecture/discussion courses, the rest of the students felt it was a positive approach because they assumed a large part of the responsibility for their learning by controlling their work schedule and the dates of completion.

From our point of view, the modular flexibility addition resulted in more students participating in the field projects; as a consequence, more were personally involved with and rewarded by an "anthropological point of view." The variable schedules also removed the tedium of talking to 50 students about the same test on the same day. And because we don't use proctors, we became more enthusiastic instructors. We now look forward to the diversity of discussing a religion project with one student, followed by discussion of an economic systems test with another. It has been a rewarding experience, well worth the many long hours of preparation.

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PHYSICAL ANTHROPOLOGY AS A LABORATORY SCIENCE COURSE IN A COMMUNITY COLLEGE

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Physical anthropology, or human biology, is one of the sub-disciplines of anthropology. As such, physical anthropology is offered in most academic institutions in the United States as a social science study program in the liberal arts and sciences. Most students who are not anthropology majors and who take physical anthropology usually fulfill part of the curriculum requirements for the social sciences; the reason is that the American Anthropological Association (AAA) maintains the general integration of the sub-grouping despite centrifugal tendencies that lead the discipline of anthropology into narrow sub-fields.[1] Despite the fact that in some academic institutions laboratory science credit is given to students who take physical anthropology, in the United States, all sub-disciplines of anthropology are traditionally considered part of the social sciences. However, if one looks at almost any course description of physical anthropology, one finds that physical anthropology is nearly always offered in conjunction with laboratory sessions, regardless of the framework in which the course is offered.

When we started offering physical anthropology at

Atlantic Community College (ACC) we also offered the course in conjunction with laboratory sessions. With this practice, my colleague, Louise Kaplan, thought of offering the course as an accredited laboratory science course rather than the customary mere reading presentation with some laboratory exercises and demonstrations. We do not claim to be unique; we merely offer to share our experience with others. At Atlantic Community College, we have always felt a need to offer students not majoring in the natural sciences another option to fulfill their laboratory science requirements for graduation. By that time, well-rounded, educated students in the liberal arts and sciences should have gained basic knowledge of themselves, of their society, and the physical universe in which they live. With physical anthropology as another choice (besides the customary biology, chemistry, mathematics, and physics), we hoped our graduates would be able to make personal and social judgements necessary for effective participation in their complex society.

Therefore, in 1969, Louise Kaplan experimentally offered physical anthropology as an accredited laboratory science course. The experimental course was successful and became a permanent one at ACC. Physical anthropology has since become one of our most popular laboratory science courses. It grew from 69 students in 1968, with one lecture and one laboratory section per semester, to 176 students in 1974, with three lecture sessions and four laboratory sessions per semester. We have offered physical anthropology as a summer course twice (1973 and 1975); these also experienced a large enrollment.[2]

At this point, it is worthwhile to note a side-effect: offering physical anthropology courses has stimulated student interest in other anthropology courses. We started with a part-time anthropologist; at present, we have two full-time anthropologists who also teach overloads (not including summer courses). Besides physical anthropology, we offer two courses in cultural anthropology and one course in archaeology. With the exception of physical anthropology, all other anthropology courses are purely elective.

Physical anthropology is one of the science courses offered to fulfill graduation requirements. Historically, ACC's first anthropology course was administered by the department of biology and chemistry because it dealt with human biology. Therefore, all anthropology courses are administered by the same department—Anthropology, Biology and Chemistry, or the ABC Department. The other anthropology courses fulfill elective requirements in the social sciences. Any one of the four anthropology courses is a basic introductory course that does not require a prerequisite. One course re-enforces and is complementary to the other ones.

We emphasize the unity of the field of anthropology and usually advocate that physical anthropologists "look at the actors while the cultural anthropologists look at the action" (Benoit, 1966).[3] We also stress the inseparability between the culture and the genetics of mankind. We attribute our student growth to the stress we put on anthropology's unity and its unique approach to the study of mankind. Physical anthropology is presented as a broad survey of the discipline. Human genetics, biochemistry, anatomy and physiology, and primatology and paleontology are all linked together to demonstrate how various biological aspects of mankind have evolved. However, the course does not stop there; it then relates how biological evolution and human culture and behavior are interwound.

We offer the course in two different formats. With our day-time students, we meet for 15 weeks, 3 days a week for one-hour lectures, and have 3-hour laboratories once a week. For our night students, we follow the same format for the laboratory sessions and meet once a week for a 3-hour lecture. Attendance at laboratories is rigidly enforced; absenteeism is not tolerated. Any student who misses more than three laboratory sessions cannot receive a passing grade for the course. On the other hand, attendance at lectures is highly recommended but is not rigidly enforced, as long as the student's performance is not below the passing grade. Course requirements also include a research paper, two hourly exams, and a final exam. The paper's topic has to be approved by the instructor.

The course is divided into three units—the first unit comprises the usual introduction to anthropology and its sub-disciplines, introduces the students specifically to physical anthropology, and covers basic concepts and theories in human evolution. The second unit leads the students towards an understanding of the dynamics of human evolution in micro- or macro-evolutionary processes. Once the students have been exposed to the theories of evolution and have studied the dynamics of evolution and population genetics, we introduce them to the time element. Now they are ready to appreciate the third unit of the course: primate and human paleontology and human evolution. The first and second units are four weeks each; the third uses the remaining seven weeks.

The laboratory is designed to aid students to understand basic concepts of biological anthropology, to enforce the lecture, and to allow students to have direct contact with the basic subject materials. Moreover, the lab gives students an opportunity to investigate anthropological and biological topics in laboratory situations. Above all, the laboratory experience functions like any other laboratory in the natural sciences: it clarifies the meaning and methods of science by giving students the opportunity to become a part-time scientist.

The first laboratory is introductory in nature, as is the first lecture. The student is introduced to laboratory procedures, the microscope, and the metric system.

During the second week, cell structure, mitosis, and meiosis are studied. The third week covers DNA and RNA, in conjunction with the lecture. By the fourth week, the students are ready to work on population genetics. The fifth week covers biochemical variations, such as the ABO, Rh, M&N, S and U blood groups, PTC tasting, sickle cell anemia, and other polymorphic traits; this session is suitable for exercises in Mendelian traits. By the sixth week, we go into protein serum testing, skin pigmentation, and dermotoglyphs. Geological time can be introduced either by the sixth or seventh week. An introduction to the human skeleton and anthropometry are the subjects of the seventh week. After learning the human skeleton in the eighth week, the students experiment in anthropometric measurements of living homo sapiens—they measure each other. Age and sex determination in skeletal remains is the topic of the ninth week. In the tenth week, the students are introduced to primatology through a trip to the Philadelphia Zoo. In the eleventh week, we show films on primate behavior and do a short exercise in skeletal taxonomy. The twelfth, thirteenth, and fourteenth weeks are devoted to fossil remains of Australopithecus, homo erectus, early homo sapiens, Neanderthal, and later homo sapiens. The fifteenth week is a study of dating and tool making, and we have recapitulation of all the laboratory sessions.

Of course, the above sequence is just a suggestion; we ourselves keep changing the laboratory subjects and trying new ideas; if they work, we adopt them. At the same time, we keep purchasing more and more equipment for the laboratory—such as an extensive collection of human skeletal remains (modern homo sapiens and fossil man reproductions).[4] Our inventory has become quite extensive, which enables us to offer more sophisticated laboratory experiments. Even with little or no equipment, fifteen successful laboratory sessions can be offered.[5]

Near the 10th week of the semester, we hand out evaluation forms to the students, which also asks for comments. Some typical remarks are: "Very interesting. At times makes me think a lot about how things came to be as they are." "Interesting learning about your possible ancestry." "The subject was very realistic to life. Held my interest." "The thing I like the most about this course is the teacher. I also like the interesting things we learn about and the interesting things we do in lab."

Of course, not all comments are as favorable as the above. We also receive notes such as: "Was over my head. It was hard to follow. I understood much better the primates and man than genetics." "Disliked learning a lot of crazy names." "Too much material presented." "Dislike 8:30 am labs." Overall, however, the positive and constructive comments outnumber the negative ones.

To sum up, we at Atlantic Community College feel that our successful experience should not be limited to us but should be adopted by other community colleges as well. There only a few colleges in the country that offer physical anthropology as a laboratory science; this number is not enough. I would like to quote one of my students, a State Trooper, who approached me after the final exam and said: "Mr. Lehavy, I do not care about my grade. I know I did better than merely passing. But I want to tell you that I learned a great deal and thank you for liberating my mind."

SYLLABUS

Scope. This course is a broad survey of the discipline of physical anthropology. Primatology, human genetics, biochemistry, physiology, and anatomy are all linked together to demonstrate how various biological aspects of man have evolved. However, the course does not stop here—it then relates how biological evolution is interwound with human culture and behavior.

Format. There will be 15 consecutive weeks of lectures and 15 consecutive laboratories. Films will be shown, discussions will be conducted, and a field trip to the Philadelphia Zoo will take place.

Attendance. All students should make every effort to attend all classes and laboratories. Absence from laboratories will not be tolerated. Moreover, the students' general progress in the course, and therefore their grades, will reflect the students' attendance and attention to these matters since exams will be related to reading, lectures, and the various class activities.

Reading. Clark F. Howell. *Early Man.* Life Nature Library, Time-Life Books, New York City, 1973. A. J. Kelso. *Physical Anthropology, An Introduction.* J. B. Lippincott Co., Philadelphia, 1974. Weiss & Mann. *Human Biology and Behavior, An Anthropological Perspective.* Little, Brown, Boston, n.d.[6]

Recommended Reading. I. Fundamentals—J. Comas. *Manual of Physical Anthropology.* Springfield IL, 1960. II. Evolutionary Theory—Charles Darwin. *On the Origin of Species.* John Murray, London, 1859. T. Dobzhansky. *Mankind Evolving.* Yale Univ. Press, New Haven CT, 1962. E. Mayer. *Animal Species and Evolution.* Harvard Press, Cambridge MA, 1963. G. G. Simpson. *The Meaning of Evolution: A Study of the History of Life and of Its Significance for Man.* Yale Univ., New Haven CT, 1949. III. Human Paleontology—C. S. Coon. *The Origin of Race.* Knopf, New York City, 1962. Kenneth P. Oakley. *Frameworks for Dating Fossil Man.* Aldin, Chicago, 1964. IV. Primatology—W. E. Clark. *The Antecedents of Man: An Introduction to the Evolution of the Primates.* Quadrangle Books, Chicago, 1960. A. Jolly. *The Evolution of Primate Behavior.* MacMillan, New York City, 1972. S. I. Rosen. *Introduction to the Primates.* Prentice-Hall, Englewood Cliffs NJ, 1974. V.

Anthropometrics—A. Hrdlicka. *Practical Anthropometry* (4th ed. by Stewart) Philadelphia, 1952. VI. Genetics & Races—S. M. Garn. *Human Races.* Thomas, Springfield IL, 1961. T. Dobzhanasky. *Genetic Diversity and Human Equality.* Basic Books, 1973. VII. Ethology—W. LaBane. *The Human Animal.* Univ. of Chicago Press, Chicago, 1954. A more complete list is available from the instructor.

In addition to these books, there are other useful sources at the ACC library. These sources include the following periodicals: *American Anthropologist, Science, Current Anthropology,* and *Scientific American.* Other useful sources, not at the ACC library, include: *American Journal of Human Genetics, American Journal of Physical Anthropology, Human Biology, Journal of Human Ecology,* and *Nature.* Howell's, Kelso's, and Weiss and Manns' books will provide the basic thread of continuity for the course, allowing the lectures the freedom to dwell on a number of issues not usually covered in the introductory texts. Hence, the students are expected to understand clearly those portions of the texts which are assigned; the lecturer will make no attempt to re-hash and may not even mention some contents of the text. Questions may be raised at any time during the lecture. Discussion is encouraged.

Background to Physical Anthropology and Basic Concepts

Week 1. The field of anthropology; background to physical anthropology; the uniqueness of mankind; and the species of mankind. *Week 2.* Formal genetics: Darwin and Mendel; and evolutionary principles. *Week 3.* Molecular and biochemical genetics; and DNA and RNA. *Week 4.* Population genetics; Mendelian laws; and Hardy-Weinberg laws.

Biological Variation in Human Populations

Week 5. The blood groups; the ABO & Rh systems; hemoglobin variations; and balanced polymorphysim. *Week 6.* Implications of human heterography; climatic adaptations; and genetic and non-genetic factors in climatic adjustments. *Week 7.* Polygenic traits; pigmentations and some other morphological characters; and skin color, hair color, and eye color. *Week 8.* Anthropometry.

Human Paleontology and Human Evolution

Week 9. Evolution and time; geological time; and the fossil record of the evolution of life. *Week 10.* Man's kinship with the animal kingdom; the division of the animal kingdom; and man's place in nature. *Week 11.* Primatology; primate behavior; and primate evolution. *Week 12.* Early hominids; and Miocene, Pliocene, and early Pleistocene hominids. *Week 13.* Homo erectus—man the tool maker; and ways of life in the Middle Pleistocene. *Week 14.* The Neanderthal man and the Neandertholoids; classical Neanderthal and progressive Neanderthal; and the early homo sapiens. *Week 15.* Modern man; and epilogue.

Exams and Requirements of Course Fulfillment. There will be two one-hour exams and a final exam, plus a research paper and three quizzes in laboratory.

NOTES

1. *Current Directions in Anthropology.* Bull. of the Amer. Anthropological Assn., Part 2, Vol. 3, No. 3, 1970.

2. Because of New Jersey's budget cuts, we were forced to offer fewer sections in fall 1975.

3. As quoted by F. E. Johnston, 1970, p. 67.

4. Fossil reproductions are available from the following sources: Carolina Biological Supply Co., Burlington NC 27215; University Museum, Univ. of Pennsylvania, Philadelphia 19104; Wenner-Gren Foundation for Anthropological Research, Inc., 14 E. 71st St., New York City 10027.

5. There are two laboratory manuals for physical anthropology available: Steegman, *Physical Anthropology Workbook,* Random House, 1974; and Stein & Rowe, *Workbook in Physical Anthropology,* McGraw-Hill, 1974. Both manuals are good, at least to begin a course with.

6. We adopted Weiss & Manns' book in the fall of 1975. It is as good as Kelso's book but also includes the latest finds.

SINGARA

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[This is my first column as editor of the Singara *department. The article by Claudia Lewis is superb but not the only kind of material I'd like to receive for the column. In it, she has presented more than an outline of a course syllabus; she has presented the context in which the course was taught as well as its purpose. Finally, she has presented an evaluation of the course by students and her response to their evaluation. I would appreciate readers' comments on her article, some of which may be printed, for I'd like to turn the column into a constructive exchange of viewpoints.]*

CROSS-CULTURAL PERSPECTIVES ON CHILD REARING AND SCHOOLING: TEACHING A FIVE-SESSION COURSE

Claudia Lewis
Bank Street College of Education
New York City

How does one introduce graduate students to some useful approaches to understanding child-life in families and communities unfamiliar to them in only five sessions? This was my problem when the curriculum committee at the Bank Street College of Education asked me to organize such a one-credit seminar, and to limit it to 12 students if I thought it best.

At Bank Street College, all students working in teacher education for an M.S. degree are liberal arts graduates studying in a program that can be completed in one year. The requirements include a heavy emphasis on child development and three credits in minority perspectives or bilingual education. My proposed "mini-course" would satisfy requirements in either of these areas.

The students are preparing for pre-school or elementary teaching or supervision, so their fieldwork assignments take them into New York City's public and private schools where they work with children of varied backgrounds (mostly Black and Hispanic). Some of the students are experienced teachers gaining fieldwork credit.

The faculty and the students are constantly reviewing the program, weighing it, and attempting to make the total offering of courses, mini-courses, fieldwork, and special opportunities a flexible one that meets the changing needs of students.

We hoped that a mini-course on Cross-Cultural Perspectives on Child-Rearing and Schooling would accomplish just what its title suggests: offering perspectives for students to followup independently; broadening value perceptions in ethnic groups different from their own; and sharpening their ability to understand child behavior in relation to the total matrix of cultural influences. Such a course could not pretend to offer a basic foundation in social anthropology, but its scope and purpose seemed appropriate, considering that although some students lack background in anthropology, all of them are now involved in a program geared in many ways to promoting insights into child and family life in various situations.

I prepared an extensive general bibliography and a smaller listing of four studies proposed for class reports. I knew from experience that the more concentrated reporting we could have, with three or four students discussing each study, the more profitable it would be for everyone. Of course, the option would remain open, for individuals to follow their own strong interests in their reading choices.

The studies selected for the general bibliography were organized under the following headings: (1) A look at children in some cultures very different from our own; (2) Studying socialization; (3) About Black families, child-rearing, and living styles; (4) Understanding the backgrounds of Puerto Rican children; (5) Changing Israeli society today; (6) Emphasis on change; (7) Schooling in transition; (8) The cultural context of learning and thinking; (9) Teaching anthropology to children.

The following is the outline of readings and discussion topics:

"Family and Childhood in a Southern Negro Community." V.H. Young. 72:2 *American Anthropologist* 269, April 1970. Characterize the main features of the child-rearing pattern described here. How does this differ from the common stereotype about Negro family life? Some points to discuss (according to your special interests): (1) What is the course of speech development and why? (2) Comment on the cultivation of aggressiveness and assertiveness. Any bearing on ways children might behave in school toward authorities? (3) Comment on the non-verbal style of communication and what we might learn from it. (4) What relationship does Young see between childhood experience and the way of life the adults grow into?

Learning to be Rotuman: Enculturation in the South Pacific. A. Howard. Teachers College Press, 1970 (especially chapters 1, 3, 4, 6, 8). (1) Explain the relationship Howard shows between children's socialization at home and the troubles they may have at school. Are you convinced? (2) What do you see as some positives of the "Rotuman character traits"? The difficulties they lead to? (3) Do you agree with the author in all of his assumptions about what American education should or might be?

The Ten Grandmothers. A. Marriott. Univ. of Oklahoma Press, 1971. (1) Sibling rivalry—do you think it existed when one brother was singled out to be the "favorite son"? Why or why not? (2) Babies not babied—implications for us. (3) What ways or beliefs tend to persist even after years of schooling and living in a white community? (4) Behavior of whites resented by Indians.

The Mountain People. C. Turnbull. Simon & Schuster, 1972. (1) Explain as fully as you can what led the Ik into such treatment of children. Refer to Turnbull's discussion on the concept of "family" in the old hunting life. Remember that in hunting societies children were something of a burden. (2) Do you see any possible threads of relationship between early childhood experiences and the values of the adults? (3) Is Turnbull convincing when he suggests that we may be heading toward the Ik way of life?

Why these particular choices? First, I felt it essential to have at least one study on child-rearing in Black

families because so many of our students work with Black children. The Young study is one of the best for breaking down stereotyped ideas and opening up new directions for thinking. The Howard study of the Rotumans wasn't chosen because it was about family life in the South Pacific, but because it seemed to offer pertinent leads for understanding child behavior in school and because its discussion of American education was challenging. Marriott's book on the Kiowa Indians was selected for two reasons: (1) our students are interested in Native Americans and their educational problems, and (2) the book offers a rich reading experience as both anthropology and literature. As for *The Mountain People*, I know of no other book presenting such challenging questions and implications, many of them relative to American family and social life.

My students included ten women and two men, ranging in age from the early 20s to the middle 40s. Several were experienced teachers. The outline seemed immediately acceptable to them, except that four students already knew they would want to choose readings from the larger bibliography in order to pursue their special interests. These choices included *Children of the Cumberland* (Lewis, 1946), "Education and Cultural Dynamics: Dahomey and the New World" (Herskovitz in Middleton (ed.) *From Child to Adult*, 1970), *Culture and Thought* (Cole and Scribner, 1973), and readings from *The Puerto Rican Community and Its Children* (Cordasco and Bucchione, 1972).

We agreed that the reports would be informal and presented as the students planned. I urged that we avoid simply summarizing the studies and try to focus on important issues, whether or not they were the topics suggested in the outline. I suggested these points be kept in mind: Effects of early home experiences and relationships; Implications for our child-rearing and schooling; Any new insights for you; Any disagreements; Any unanswered questions.

The reports were extremely stimulating. All the students were capable of independent work and were experienced in discussion participation. The readings suggested questions that a number of students hope to pursue on their own. Only the study of the Rotumans seemed a little lacking in substance because it didn't offer enough details on child life in the family.

The written evaluations left expressed great enthusiasm and the hope that the course could be extended. But even five sessions accomplished some of the basic aim: Students wrote, for instance, "All of it was extremely useful and stimulating. I had not been exposed to the discipline of Anthropology and you have whetted my appetite. I plan to continue exploring the bibliography." "Fascinating and full of sharing. Efficient in covering even more than in the master plan." This comment went right to the heart of the course goal as I conceived it: "I have been really enjoying the course as well as finding it growth-facilitating both personally and intellectually."

PROFESSIONAL NEWS

CAE PROJECTS FOR 1976 AAA ANNUAL MEETING

Symposia being sponsored by CAE at the upcoming meeting are: (1) *Cross-National Approaches to Education and Change*, Thomas J. La Belle; (2) *Sex, Class, and Ethnicity: Females and the Educative Process*, Judith Preissle Goetz; (3) *Power Processes in Education: Theoretical and Empirical Perspectives*, Angie M. Guggenberger Nelson; (4) *Community-School System Collaboration in the Development of Multicultural Education Programs in Chicago*, Elena Berez-Aluce Mulcahy, Jean J. Schensul, and Maria B. Cerda; (5) *A Study of Functional Language in the Classroom*, Roger W. Shuy.

Roundtable Luncheon topics will be: (1) "The Anthropology of Education: Evolution of an Area of Inquiry," Frederick Erickson, Harvard (speakers: Solon Kimball, George Spindler); (2) "Ethnographic Approaches to Evaluating Schools," Woody Clark, UCB; (3) "The Scope and Limits of Education in Producing Development in the Third World," Thomas Labelle, UCLA; (4) "Biculturalism: Efforts and Prospects for the Spanish-speaking in the U. S.," Henry Torres-Trueba, Illinois; (5) "Population Issues in Anthropology and Education: A Discussion of the Economic and Demographic Perspective," Kathleen J. Adams, Central Washington State; (6) "Community Development and Education: Education for What?" Thomas Labelle, UCLA; (7) "The Teaching of Anthropology," John D. Herzog, Northeastern; (8) "Curriculum Materials Workshop," sponsored by publishers for Washington and East Coast teachers of anthropology in elementary, secondary, and community college settings.

PLAY GROUP MEETS IN ATLANTA

The Association for the Anthropological Study of Play (TAASP) held its second Annual Meeting in Atlanta, 31 March to 3 April, in association with the Southern Anthropological Society and the American Ethnological Society. Brian Sutton-Smith, Teachers College, Columbia, was selected as Publications Editor; and three new members-at-large, to serve two-year terms, were elected: Elinor Nickerson, San Ramon Valley High School, Danville CA; Allen Sack, New Haven; and Helen Schwartzman, Institute for Juvenile Research, Chicago. Officers continuing to serve for one more year are: President, B. Allan Tindall, Univ. of California–Berkeley; President Elect, Phillips Stevens Jr., SUNY–Buffalo; Immediate Past President, Michael A. Salter, Windsor; and Secretary-Treasurer, Alyce Cheska, Illinois–Urbana. Membership in TAASP is open to all individuals and institutions interested in furthering the study of play. Information regarding membership and publications (Quarterly Newsletter, Proceedings, and so forth) may be obtained by writing to Elinor Nickerson, Box 297, Alamo CA 94507. Fees are $10 Regular, $5 Student, $20 Institution, and $200 Life.

TWO VACANT POSITIONS IN PAPUA, NEW GUINEA

Audio-Visual Aids Officer. Salary is $U.S.18,500. Appointment is to a headquarters position in the national capital, Port Moresby. However, some travel to various centers and schools is essential: this would require absences of one or two weeks at least four or five times a year. Housing is adequate but not luxurious. Cost of living in national capital is higher than elsewhere in the country. (1) Duties: develop audio-visual aids as support material from design through preparation and trial stages. Expensive materials or ones that rely on expensive equipment are not practical in Papua, New Guinea. Cheaply produced graphics, radio, and aids are the basic areas of activity. Priority activities are the community school curriculum (primary) and provincial high schools (lower secondary). (2) Qualifications: professional qualifications, experience in education, and tertiary-level study in an area related to the development of audio-visual aids. Experience in a developing country important. Ability to identify needs quickly and to get approved plans into speedy execution.

Coordinator of Material Development and Evaluation. Salary is $U.S.23,500. Appointment is to a headquarters position in the national capital, Port Moresby. However, travel to various centers and schools will be essential. A lot of this will be by air as Papua, New Guinea, is a rugged mountainous country. Housing is adequate but not luxurious. Cost of living in the national capital is higher than elsewhere in the country. (1) Duties: responsible for reviewing and controlling the development of all educational learning aids from Papua, New Guinea schools, but particularly the community (primary) schools and provincial (lower secondary) high schools. Printed materials and, to a lesser extent, radio programs are the major areas of activity. The materials required have to be cheap, simply written, and structured to minimize the programming tasks of teachers. Responsible for evaluating existing materials in terms of internal efficiency (comparison between materials) and external efficiency (alternative expenditure items to material production). One project for provincial high schools could be provision of student materials to support integrated generalist approach to teaching. Responsible for evaluating success of various curricula. (2) Qualifications: very high level qualifications and considerable experience (some of which should be in a developing country) in the development and evaluation of educational materials and curricula. Able to learn the details of existing systems and materials quickly and to organize an effective contribution to further developments and reviews.

Apply to Dennis Donahoe, Superintendent, Curriculum Unit, Department of Education, Konedoba, Papua, New Guinea.

DEMOGRETICS AS THE DETERMINANT OF DEMOGREMICS: A COMMENT ON HOWARD [Quarterly, May 1976]

Is demographic socialization important? If so, does its investigation require naive (theory-less) research? Alan Howard has argued a "desperate need" to observe the individual forming attitudes toward density, migration, and other populational analysis. I welcome his emphasis on this neglected dimension but suggest that our approach to it should be (1) hypothesis and theory, then (2) codification of existing data toward those theories, and only then (3) some sort of experimentation for validation. Space limits me to aspect one.

After 100 years of anthropology, we have no excuse to begin any problem "cold." Our best present paradigm–"cultural materialism"–argues that a culture's control of its environment ultimately decides its social

organization. The latter fundamentally governs its ideology, which in turn basically directs its logic. I have elsewhere[1] named these three stages as the Steward, Taine, and Gladwin effects, respectively.

The native's viewpoint, often termed emics, is therefore ultimately delimited (though not necessarily step-by-step) by the broader techno-environmental factors perceived by the outside comparative scientist—by what is often termed etics. Howard has highlighted the insider's populational view, so let us christen that component of demographic emics as "demogremics." The theory suggests that it must ultimately obey the ecological-economic pressures on demography, which we may name "demogretics."

Now, a major evolutionary trend is the increasing conversion of inorganics to organics (to biomass). The ongoing Darwinian/Malthusian rivalries force each individual and group toward optimal environmental adaptability in a system called "agonemmetry."[2] No society can long lower its reproductive rate seriously below its neighbors or it will siphon them in.[3] The current zero population craze is but the interplay of improved longevity through medicine plus the inability of the automated Western economy to utilize human potential.

Much of the past decade's cultural ecology fieldwork has reported both the techno-environment and enculturation, although rarely intertwining them. Educational anthropological codifiers should correlate them, beginning always with the techno-environment. We may anticipate the findings: Ideas about fertility and other demographics are not self-generating but tend to obey economic opportunity. Thus, in the stagnant U.S.A. of 1970, undergraduates on my campus celebrated Earth Day by distributing leaflets that snarled, "On E-Day, the UMKC Student Action League would like to remind you that pregnant women are ugly!" Many sparse prehistoric cultures, by contrast, glorified fertility, as we can see in the lusty proportions of the Venus of Willendorf statue.

As with fertility, migration behavior will be found to obey densities and power politics; attitudes are a mere lubricant. Thus, just two centuries ago, a free 11,000-mile ship passage to Australia was offered, indeed forced, on people convicted in London of stealing a gob of bread. The expense cannot be explained by the thief's book-learning but by the British industrialists' desire to Anglicize a Pacific colony.

Although the behavior flow is from survival needs to ideology, the latter is no mere mirror image. Scientists of sub-cultures err in extrapolating from rat colony disasters the densities humans can withstand. But human culture is a creative optimizer. Thus, the multi-national firms' need for cheap labor, plus Bamboo Curtain refugees, make today's Hong Kong factories prosper. The island's compactness is 100 times that of New York City; it is made tolerable through the emergence of seclusion customs such as shunning a neighbor's portion of the common kitchen. So Hong Kong enjoys further immigration, not emigration.[4]

Nor need these demographic orientations arise traditionally or spontaneously. "Ad-mass" is appearing even in Indonesia, where the Information Ministry has infiltrated the rural *ludruk* folk-skit with songs advocating forceful migration into West Irian.[5]

And so Howard is to be congratulated for annexing this worthy subject to educational anthropology. Folk demographies are not arbitrary but techno-environmentally obedient. As Malinowski (1926, 1948) crowed, myth "is not an idle tale, but a hard-worked active force." At this stage of our subdiscipline's evolution, we should harness "demogremics," not through naive ethnography but through cultural-materialistic codification.

Henry G. Burger
University of Missouri

Notes

1. *General Systems*, volume 20, page 107, 1975.
2. *General Systems*, volume 12, page 209, 1967.
3. See "The Tragedy of the Commons," 162 *Science* 1243, 1968.
4. *Urban Anthropology*, volume 1, page 144, 1972.
5. *Proceedings*, American Ethnological Society, page 69, 1966.

References

Malinowski, 1926 [no title given].
Malinowski, *Magic, Science and Religion*, 1948.

PUBLICATIONS

[Ed. Note: This section of the *Quarterly* has been revised in order to assist libraries and CAE members to develop a continuously updated bibliography in anthropology and education by means of annotated references which have *not* been included in the Burnett et al. *Anthropology and Education: Annotated Bibliographic Guide*, nor in previous issues of the CAE *Quarterly*. An additional section will list materials useful in teaching anthropology.]

Anthropological Study of Education

Beeby, C. E. *The Quality of Education in Developing Countries.* Cambridge: Harvard Univ., 1966.

Examines the implications for quality development in educational planning, a problem largely ignored, the author maintains, in the concern with expansion of educational programs in developing nations. Includes analysis of the concept of quality in educational theory, the roles of educator and economist in educational planning, and factors related to education's conservatism in the face of social change. A model of developmental stages through which educational systems must pass is offered in an attempt to understand the nature of these educational problems.

Brembeck, Cole S. "Education for National Development." *Comparative Education Review*, Vol. 5, No. 3, 1962.

Some of the basic causes of resistance to change and the results of this resistance on educational institutions are examined. Through the impact of technological change, changing status relationships, and political change on schools in Asia, the author discusses some of the basic problems facing educators in developing nations and makes some suggestions for implementing change. Also included is a discussion of needs in educational planning in the areas of instructional methods, teacher preparation and placement.

Catala, Pierre. "Education and Rural Development." In *Prospects in Education* No. 2, Paris: UNESCO, 1969.

Modernization and expansion of educational systems are discussed in terms of potential problems rising from long-term objectives and changes in educational methods. The expansion of Western industrialization is suggested as promoting a cultural orientation which may conflict with economic realities. The importance of careful educational planning is emphasized to reduce uneven development.

Klineberg, Otto and Maria Zavalloni. *Nationalism and Tribalism Among African Students.* Mouton, Paris: Publications of the International Social Science Council, No. 12, 1969.

Shifts in attitudes towards allegiance from tribal to national groups forms the basis for examining the process of national integration in African states. Employing a sample of university students from throughout Africa, attitudes and behavior towards national or local identity were surveyed. Findings indicated a general but uneven trend towards national integration. The artificiality of national boundaries and the relative newness of independent status were seen as contributing to this pattern.

Teaching Anthropology

Cover, Lois B. *Anthropology For Our Times.* New York: Oxford Book Co., 1971.

Written as a text for secondary and community college students, this book provides a broad inventory of general anthropological data and concepts. Some effort is exerted to relate the general anthropology to events in modern times. At the end of each chapter is a glossary of terms, study questions, and a list of recommended reading.

Rosenfeld, Gerald L. "Anthropology as Social Studies in the Elementary School." 69:8 *The Record* 767, May 1968.

This brief paper focuses on the relevance of anthropology to elementary education. Both the content on social studies, and the fact that children may be viewed as new cultural participants much like anthropologists are argued as reasons for incorporating anthropology in the elementary social studies curriculum.

Thomas, Georgelle. "Programmed Instruction for Teaching Anthropology in the Fifth Grade." 36:4 *Journal of Experimental Education* 88, Summer 1968.

This paper compares achievement levels of fifth grade students taught anthropology using programmed instructional materials with those taught anthropology by conventional classroom techniques. No significant differences in achievement levels are found between groups in terms of race, sex, or reading ability. The author suggests that less able readers may be no less handicapped in programmed instruction than in conventional instruction.

Wilson, Paul. "World Cultures: A Matter of Method." 43:8 *The Clearing House* 501, April 1969.

This paper focuses on the problem of over-specialization in presenting material on cultures around the world. Four procedures: the "Semester Approach," the "Comparative Approach," the "Area Approach," and the "World Problems Approach," are presented as ways to structure the presentation of such materials.

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